PUBLICATIONS

OF THE

MASSACHUSETTS MEDICAL SOCIETY,

VOL. III.—No. I.

Myxoma, or Hyperplasia of the Villi of the Chorion.

BY ALEXANDER D. SINCLAIR, M.D.

OF BOSTON.

Formation and Significance of Renal Casts.

BY ROBERT THAXTER EDES, M.D.

OF ROXBURY.

Physiological Action of Bromide of Potassium and Ammonium.

BY ROBERT AMORY, M.D.

OF BROOKLINE.

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PRINTED BY DAVID CLAPP & SON.....334 WASHINGTON ST.

MEDICAL AND SURGICAL JOURNAL OFFICE.

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The expense of printing the "Publications of the Massachusetts Medical Society" is defrayed by a Fund devised to the Society by the late Dr. GEORGE C. SHATTUCK.

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MYXOMA, OR HYPERPLASIA OF THE VILLI OF THE CHORION.

By ALEXANDER D. SINCLAIR, M.D.

OF BOSTON.

READ JUNE 1, 1869.

MYXOMA, OR HYPERPLASIA OF THE VILLI OF THE CHORION.

Myxoma, or Hyperplasia of the Villi of the Chorion, are terms expressive of a morbid condition which has hitherto attracted but little attention from pathologists. Hypertrophy of one or more villi of the chorion has been occasionally noticed. Nothing like extensive growths, however, of this nature are mentioned in works on Midwifery or Diseases of Women. The ordinary hydatidiform or vesicular mole is familiar to all of us, either from the description of writers, or the actual observance of specimens which are not very infrequent in their occurrence. A condition of things resembling the specimen to which I am about to direct the attention of the Society is described by Virchow in his work on the Pathology of Tumors, under the title of fibrous myxoma of the placenta. But the extraordinary specimen described and figured by this learned pathologist was comparatively limited in extent, for it was confined to one of the cotyledons of an otherwise healthy placenta of a seven months fœtus: whereas in the specimen now under consideration all of the villi of the chorion are implicated in this remarkable hypertrophy. The distinguished pathologist, already referred to, attributes abnormal conditions of the chorion villi to an endometritis of the decidua; and has observed, in several instances, inflammatory thickening in the caduca, or membrana decidua of the vesicular mole.

Mrs. ——, æt. 40, mother of seven living children, young-

est about four years old, aborted after her first and fifth conceptions about the third month. She married at the age of nineteen years, since then has suffered, more or less, from indigestion and constipation, but has had a fair appetite and continued moderately fleshy. Catamenia, regular, with some pain, were last present on Jan. 27th, 1868. Not long afterwards I was called to visit her, and found her suffering from nausea, heartburn and general discomfort of the stomach and bowels; no vomiting. Having experienced similar sensations after former conceptions left little doubt in her mind that she was again pregnant. Except the discomfort already alluded to, nothing remarkable occurred until the 10th of May, when she noticed, for the first time, her linen slightly stained with a somewhat bloody matter which appeared at intervals, for the next four days, with a slight pain occasionally felt in the uterine region.

Near midnight on May 14th, she was suddenly seized with an alarming hæmorrhage, causing her to faint and become pale, as if dead. She had recovered her senses before I arrived, but the pallor which remained together with the appearance of the bed on which she lay, bore evidence of a frightful loss of blood.

On examination, the uterus felt like that of a woman in the fifth month of pregnancy; the os rigid, though sufficiently dilated to admit the index finger. Hæmorrhage had ceased. Careful examination of the clots discovered only a small fœtus having the appearance of full three months growth. Restoratives were used and the vagina plugged to prevent further hæmorrhage. The uterus remained quiet during the remainder of the night, and on the following morning the tampon was replaced by a fresh one and ergot administered. The uterus appeared to partake of the general physical prostration consequent upon the profuse hæmorrhage, and it responded more feebly to the influence of the

means employed than might be expected in an organ so largely developed. A large sponge tent was then substituted for the tampon, and the ergot continued. This dilated the cervix and excited uterine contractions, expelling large quantities of bloody fluid, and *opaque, flattened, flesh-colored bodies, irregular in size and form, many of which looked like pieces of decidua, generally longer than they were wide and measuring from about three to ten or twelve lines in length; others consisted of tuberosities strung together by more or less elongated and constricted portions. Many of these strings measured from two to five inches in length.* All of these bodies were covered with a thin membrane (exochorion), from which sprung numerous bud-like processes, varying from one to three lines in length.* Nowhere did these growths present the vesicular appearance which characterizes hydatidiform degeneration of the chorion villi, and when placed in a basin of water they sank to the bottom like pieces of flesh. Sponge tents and ergot were repeated at intervals, followed by discharges similar to that described, though gradually lessening in quantity, but the last of these bodies were not expelled until the eighteenth day from the time of the first hæmorrhage. No membranes were found, although these might have escaped notice among such masses. The quantity of these bodies discharged would more than fill a quart measure.

On microscopic examination of these bodies (which had been in alcohol for nearly twelve months) they were found to consist, essentially, of mucous tissue, or more literally, cells, with here and there an indistinct fibrous appearance. Fatty metamorphosis had commenced in the tissue. The largest tuberosities contained masses of blood corpuscles and granular debris. No bloodvessels were found, although searched for diligently.

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* Vide Plate, Fig. A, B, C.

Since the time of Aristotle, some writers have asserted that degeneration of the chorion villi is consequent upon the death of the embryo, while by others the contrary of this is maintained. Without entering into discussion, this one fact may be pointed out for the benefit of those who do not believe pathological changes of the chorion villi the result of the death of the embryo; viz., that before us is a fœtus of full three months development, accompanied by one of the most remarkable conditions of the chorion villi perhaps ever observed.

HYPERPLASIA OF CHORION VILLI.

OUTLINES OF THREE VILLI (OR MAY BE ONLY PORTIONS OF VILLI).

A Nearly five inches in length, and one inch in greatest breadth; one-fourth inch greatest thickness.
B About one-sixteenth inch in thickness.
C Another form of villus.

FORMATION AND SIGNIFICANCE OF RENAL CASTS.

BY ROBERT THAXTER EDES, M.D.

OF ROXBURY.

READ JUNE 1, 1869.

FORMATION AND SIGNIFICANCE OF RENAL CASTS.

Almost every author who has written upon the subject has employed a different system of classification for the various forms of renal disorder embraced under the head of Bright's disease, some supposing that a greater or less number of distinct diseases exist, and others regarding the various pathological changes found after death as stages of one disease.

The modern systems, which differ much less from each other than did those prevalent some years since, distinguish at least three separate diseases, which have, however, much in common with each other.

I propose here merely to speak of a few points in regard to symptoms common to the whole group.

The structure of the kidneys, although in its details somewhat more complicated than that of many glands, is essentially simple. The plan is the usual one of capillaries surrounding a tube lined with secreting cells, but is modified by the development at the extremity, of a dilatation which allows a tuft of capillaries to project into the interior, forming the so-called Malpighian bodies. These bodies form, in the injected specimen, very prominent and noticeable objects, which are arranged with considerable regularity in rows radiating from the pelvis of the kidney.

Their physiological function, as distinct from the remainder of the tubes, is not known. It has been supposed that

they secrete, or rather filter, the water, while the more solid constituents enter the secretion through the walls of the tubes. This view, although plausible, is pure theory.

For our purposes, however, they may be left out of the account, as there are no means of diagnosticating any affection of these bodies; and, indeed, anatomy shows that with the exception of the waxy degeneration, the principal change affecting them is atrophy, and that secondary.

The tubes are composed of an external, thin, transparent, structureless membrane, lined with a single layer of cells. They are much convoluted in the cortical substance of the kidney, but become straight when passing through the cones to open into the pelvis.

Some, and perhaps all, form one or two loops in the straighter portion, a tube passing down parallel to the general direction of the pyramid, then making an abrupt turn and passing outward before joining the larger common ducts.

A very important fact in regard to the size of these tubes has only lately been brought to light, and has, so far as I know, received no consideration in its bearing upon pathology. The portion of the tube immediately adjoining the Malpighian bodies—that is, the upper extremity—is large and filled with large epithelial cells of a somewhat different character from those below. The portion which makes the loop just mentioned, is smaller and lined with smaller epithelial cells. Superior importance has been attributed to the upper portion of the tubes, as being more actively employed in elaborating the secretion of the gland, while the lower portions are supposed to be chiefly useful as conductors. This is, however, principally theory.

The character of the epithelium lining the tubes is important to be remembered. It consists of only one layer, differing from that of most other mucous surfaces where there are several, the lower growing to make up for the casting

off of the older and superficial. Some secretions are furnished by a breaking up of a luxuriant growth of cells, the younger at the bottom of the tube continually pushing up the older, which, being ruptured, discharge into the glandular cavity their peculiar contents. Such are the sebaceous, mammary and seminal secretions, and probably many others.

So far as we can see, it is anatomically almost impossible for the renal secretion to be of this character, and yet strong arguments could be brought to show that something of the sort does really take place.

If physiology were all we had to consider, the action of the secreting cells in the kidney could be very easily explained. We might say that the constituents of the urine simply filtered through, and that the same cells always remained within the tubes, or, at all events, were very slowly changed.

There is no renal epithelium to be found in healthy urine; but this would hardly be conclusive on the point, as the cells might be ruptured, discharging their contents before reaching the bladder, and their remains be unrecognizable. Some pathological facts, however, throw doubt on this explanation, and make it appear extremely probable that a regeneration of the lining of the tubes must, in some places at least, take place, or else that a considerable portion of this lining can be spared with but little detriment to the organ.

The fibrous stroma interlaced among and supporting the more important and delicate secreting structures of the kidney is much less considerable in amount and bulky in structure than it appears in some drawings and in some specimens, especially those in which the bloodvessels are not injected. When the vessels are well filled with injection and the epithelium remains in the tubes, it may be seen that the unoccupied space between the lining or cellular contents of the

different tubes is very small, it becoming evident by this mode of demonstration that a large part of the stroma apparently existing in sections is really empty bloodvessels and basement membrane, and that the actual amount of connective tissue is quite small.

Beale considers the apparent increase of fibrous tissue in the contracted kidney, which some call an exudation and others cirrhosis, to be simply the remains of collapsed bloodvessels and basement membranes of tubes which have lost through disease their epithelial lining.

It is natural that we should look to the excretion for data by which to judge of the condition of the organ which furnishes it.

The connection of albuminuria with disease of the kidneys has been known since the discovery of Dr. Bright; but we ought now to take into consideration not only the abnormal constituent, but also the quantity of water and of urea which are excreted, and especially to watch for that débris of the kidney which tells somewhat of the nature and amount of its disorganization. This débris consists mostly of so-called casts, composed either of substances effused from without into the tubes, or of the lining itself of those tubes in a more or less altered condition, or of both together.

The principal form in which the extraneous casts, or those not composed of altered epithelium, are found, is that of the hyaline, waxy or fibrinous casts; and of these names the first is the best, as it neither involves an unproved theory like the third, nor suggests an erroneous pathology like the second. They are often difficult to detect, from their extreme transparency and feeble outlines. They may be of various diameters. Blood casts are occasionally met with, but less frequently than any of the other kinds.

When the epithelial lining of the tubes is discharged into the secretion, it sometimes occurs as separate cells, as small

groups, or as larger aggregations in the form of casts. It is probably to degeneration of epithelium that the granular and fatty casts are to be referred, the component cells having undergone more or less complete degeneration. It is usual also in the hyaline casts to meet with more or less epithelial cells, granular matter or fat.

It has been supposed that the size of the casts was an indication of the presence or absence of the epithelium lining the tubes, a small cast being of course found within the cavity of a tube whose epithelium remained upon the walls, while a larger one is supposed to show that only the basement membrane of the tubes remains.

Observations made within the last few years, showing not only that the tubes vary greatly in size, but also that between the larger tubes of the cortical portion and their outlet intervenes a portion of smaller calibre, make it very doubtful whether such indications can be relied on, and also whether the casts which appear in the urine arise at all from the convoluted portions, or merely from the straighter tubes near the pelvis of the kidney.

It is certainly difficult to see how a cast formed within the basement membrane of a convoluted tube, including, that is, the whole epithelium, can be washed through a long passage considerably smaller than itself, as it must be to reach the outlet. A smaller cast, formed within the layer of cells, might, however, be so, since the measure within the cells varies less than that of the enclosing membrane, the greater diameter of the large tubes depending on greater thickness of the cells.

Nevertheless there must be certain large casts, of which it would be pretty safe to say that they came from a large tube deprived of epithelium, and also some small ones apparently from uninjured tubes. I am sorry that I am able to give no measurements from my own observation.

No one kind of casts is characteristic of any one form of Bright's disease; unless, perhaps, the fatty, which is more rare, may be said to indicate a chronic affection. They are not, however, without significance in this respect. Great abundance of epithelial casts and of broken down epithelium belong rather to the acuter forms, while the granular and fatty are more likely to be the products of chronic degeneration.

One of the most practical questions in this matter is, whether the presence of casts, of any or all kinds, indicates irreparable lesion of some portion of the kidney; and another, supposing the tubes from which the casts actually come to be disabled from further action, whether it follows that the process must go on until too little of the kidney is left to perform its functions. I think we may give favorable answers to these questions on various grounds.

As to the first question: on anatomical grounds we know that many casts are formed inside of the epithelium, this remaining attached to the tube. The size often shows this, and I have seen, in a diseased kidney, a solid mass filling the inside of many tubes, the lining cells, although present, being thin and small.

It is a question whether this atrophy might not be produced by the pressure of the effusion within the tube, somewhat as atrophy of the lung is caused by pressure from effusion in the pleura.

Casts formed in this way must principally consist of effusion, and not of the natural contents of the tube. They are hyaline, but sometimes include a little epithelium, a few granules, or fat globules.

The most conclusive answer to the first question, however, comes from clinical experience. It is well known that albuminuria is a very frequent accompaniment of scarlet fever. It has even been said that albumen will

always be found, if looked for carefully through the whole course of the disease. It is by no means improbable that such is the case with regard to casts also. They are certainly very frequently present. Possibly the discharge of renal epithelium into the urine is analogous to the desquamation of the cuticle, and goes on just as normally and physiologically as the former process, being undetected because not looked for, and harmless unless other circumstances interfere with its proper course, or unless excessive in amount.

The condition of the kidney when casts occur may be and probably is usually but little more than that of congestion.

A little boy died of pneumonia about three weeks after having scarlet fever. He had no œdema and no convulsions. At the autopsy there was no external œdema, but the walls of the gall-bladder and several folds of mesentery were filled with serous effusion, and there was also effusion in the pleura. The urine in the bladder contained casts, and the kidneys were congested in the pyramids, as usual in acute dropsy.

Some months ago I examined the urine of a young man convalescent from an attack diagnosticated by his physician as scarlet fever, though with almost no eruption, and found several hyaline casts. He seemed perfectly well, but weak, and has had no symptom of renal trouble since. The cuticle was peeling off his hands at the same time.

This fact of the great frequency if not invariable occurrence of casts, often in great numbers, and also of separate epithelial cells in the urine of scarlatinous patients, taken in connection with another well known fact that although many cases of dropsy or convulsion dependent upon renal disorder occur after scarlatina, yet a great many cases of scarlatina, and severe cases too, recover without such symptoms, shows that the occurrence of casts does not make the prog-

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nosis of uræmic symptoms a necessary one, nor, when such symptoms have occurred, justify very unfavorable predictions. The prognosis of uræmic convulsions after scarlatina is usually quite favorable, by far the larger proportion of cases recovering without permanent, or, at any rate, very material injury. Idiopathic acute dropsy stands in very nearly the same category.

Another class of cases, in which not less importance but a less favorable prognosis attaches to the presence of casts in the urine, occur in the puerperal condition. The connection of puerperal convulsions with albuminous urine and a certain amount of disturbance of the kidneys, is a generally admitted fact, though all the steps of the causation are by no means satisfactorily made out.

How large a proportion of puerperal women would show a deposit of urinary casts at some time or other, whether there were cerebral symptoms or not, is a subject as yet but little investigated, and demanding much time and patience.

A case or two will illustrate their occurrence without symptoms so severe as might have been anticipated, and one of them under peculiarly unfavorable circumstances.

A hard-working Irish woman, the mother of ten children, had a severe attack of erysipelas extending up the right arm from the thumb, and the hand with a considerable part of the forearm became gangrenous. On the sixth day the urine was slightly albuminous and contained a few casts. That evening she was delivered of a five or six months fœtus. Two days after, her urine contained no albumen, but casts, granular and transparent, containing each a few epithelial cells. Here were apparently three conditions having very perilous relations to each other.

In the first place, erysipelas has been considered very closely allied to some forms of puerperal fever. Then the condition of the kidneys usually supposed to be connected

with the presence of casts makes the prognosis of erysipelas unfavorable in the highest degree.

Erichsen says, "The most dangerous complication of erysipelas, and one which, when it exists, almost precludes the hope of recovery, is a granular state of the kidneys, with albuminuria. I have never seen any patient laboring under this disease and attacked with erysipelas escape with life; the sloughing and suppuration running on unchecked by any treatment that could be adopted."

The same condition also is one of the last which the surgeon wishes to see in a patient on whom he is to operate.

The sequel of this case was that there was never a bad symptom, or, in fact, any symptom at all worth mentioning on the part of the uterus or the peritoneum, that the arm was amputated, the stump healing quite well, and that the patient is now a healthy, strong-looking woman.

I presume the recollection of those here present would furnish many examples of the occurrence of albumen and casts in cases of puerperal convulsions, the patients afterward recovering.

I have several times examined, for a friend, specimens of urine which have been albuminous and contained casts, from a patient who had puerperal convulsions, but who is now about, and considers herself nearly well, the abnormal constituents having been detected long after her convalescence and up to the time of the last examination.

It is in the ordinary chronic form of Bright's disease that casts assume their highest importance. How frequent they are, how serious they ought to make the consideration of any case in which they occur, and their significance in the various forms of disease of the kidneys, are points which have recently received great attention in the recent literature of the subject, especially in the works of Beale, Dickinson, and Stewart.

Yet even in these cases we know that patients may live for years after their appearance, in comfort if not in perfect health, their health being rather precarious than actually infirm.

The casts may, as is well known, disappear even in chronic cases, and it is not beyond the bounds of possibility that this disappearance may coincide with an arrest of the disease, or, at any rate, a retardation of its progress nearly approaching thereto.

This brings us to the second question, as to the localization of a lesion in one part of the kidneys.

A very marked distinction in the degree of fatty degeneration of cells lying almost adjacent to each other in the same tube may sometimes be seen, and it is quite common to find great differences in the condition of neighboring tubes, some being much more diseased than others.

In the cirrhotic or contracted kidney, there are found, especially though not exclusively near the surface, groups of much atrophied tubes and Malpighian capsules enclosed in a granular mass of exudation or fibrous tissue, while the neighboring tubes are but slightly if at all affected. In a specimen of this kind the whole organ may be much reduced in size, the surface rough and uneven, the atrophied portions above mentioned numerous, and yet the inner portion of the cortical substance apparently healthy. It is probable that such cases may have been going on for years with but very little disturbance of the health, and the condition of the kidneys only becoming known at the autopsy, death taking place from complications whose connection with the original disease was not suspected. Clinical evidence of an arrest of the morbid process, or a retardation amounting to nearly the same thing, it is hardly necessary to adduce.

Does it follow, if these views are correct, that the occurrence of casts is a symptom of no consequence, and that it

is a waste of time in such cases to spend it over the microscope?

I think it is rather the contrary; since a diagnosis at a stage when good results may be looked for from judicious treatment, is of more importance than the confirmation of an already sufficiently unfavorable prognosis.

These data must be taken into the account just as others derived from excreta are, and a diagnosis and plan of therapeutics founded on them, together with all other symptoms of the special case.

Some analogies which may be drawn between renal and pulmonary pathology are in many respects quite close.

Fibrinous casts may form in the bronchial tubes, as in the uriniferous, and it is possible that they may in the latter case, as in the former, leave the proper lining intact.

It is the fibrinous effusion coagulating in the larger tubes which is discharged from the air passages in consistent pieces of greater or less size in croup and in fibro-bronchitis, while the exudation of pneumonia, occupying the extreme air cells, never becomes evident in fragments bearing the form of the cavity in which they were formed. So from the peculiar form of the renal tubes already described, and from the clinical tendencies of scarlatina, acute dropsy, and possibly the puerperal condition, it seems probable that a large part of the masses of epithelium found in the urine take their origin rather from the more capacious tubes near the point of discharge into the pelvis of the kidney than from those more intimately connected with the secretion and forming the most important part of the organ.

The importance of the discovery of casts in a case of scarlatina or a puerperal case, is somewhat similar to that which might be attached to the occurrence of viscid bloody sputa in a case of some serious chronic disease, or after an operation. They indicate a condition calling for watchfulness and care, but one which may be perfectly recovered from.

In idiopathic chronic cases their meaning might be compared to that of the shreds of lung tissue which have occasionally, by patient search, been found in the sputa of phthisis. Their occurrence is then a more serious matter, but as we know that a tubercular cavity may heal and leave only a scar or hard nodule, so we may hope that only a part of the kidney may be destroyed and the remainder be sufficient, if carefully treated, for its duties.

No one can be better aware than myself of the imperfect and fragmentary character of these observations. A part of the imperfections, however, attach necessarily to the subject. The physiology of the kidney, to say nothing of its pathology, is yet incomplete; and before we can be sure of the precise interpretation to be put on all the symptoms, we must know better how the organ acts in health.

There is, however, a large field open for practical men in the clinical study of the symptoms of renal disease.

The best observations for determining the natural history of Bright's disease in its various forms must come from private practice. So large a number of cases go through the earlier stages in a condition in which one would not think of applying for admittance to a hospital, and the minor disturbances being of such a kind as to come under no medical eye but that of the family physician, it is from them that we can get the most thorough and minute information, if cases are studied from the beginning with the microscope and the test tube.

It is in these earlier stages that medical treatment is of most avail, and it is now recognized as more efficient than it was formerly thought to be.

The practical lesson of what I have said may be expressed in a very few words.

Look early and thoroughly for casts, but do not despair of your patient if you find them.

EXPERIMENTS

UPON THE

PHYSIOLOGICAL ACTION OF BROMIDE OF POTASSIUM AND AMMONIUM

(As determined on Man and the Lower Animals).

By ROBERT AMORY, M.D.

OF BROOKLINE.

READ JUNE 1, 1869.

PREFACE.

With one or two exceptions the experiments to be related in this paper have been conducted by myself or under my supervision, of which a detailed description has been given; because the study of bromide of potassium is here considered from a point of view different from that usually pursued in the study of drugs.

The length of time I have occupied upon this subject should certainly demand a patient hearing. You may perhaps be astonished if I should tell you, that for eight months my mind was occupied by these experiments. It was with the advice and promised assistance of Prof. E. H. Clarke, that I began this work, but a variety of reasons have conspired to prevent the fulfilment of this promise; and it is with much reluctance, and feeling that the therapeutical considerations have been unfortunately omitted, that I have to finish my work alone.

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Chapter III.

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THE PHYSIOLOGICAL ACTION OF BROMIDE OF POTASSIUM AND AMMONIUM.

CHAPTER I.

Bromide of Potassium is absorbed readily by the mucous membrane of the mouth. This proposition would be illustrated by the following experiments:

Exp. I.—Under the effects of ether, a dog was operated upon for ligature of the œsophagus. When this had been accomplished, a solution containing 160 grains of this salt was placed in the mouth of the animal. In ten minutes after the contact of the drug with the buccal mucous membrane, blood ʒ j. was drawn from the carotid artery, and, examined by means of chlorinated water and bisulphide of carbon, gave the peculiar reaction of the liberation of bromine, viz., a reddish yellow color.

The bromide could only have been absorbed by the mucous membrane of the mouth and pharynx, for great care was taken to prevent the contact of the solution with any other tissue.

Exp. II.—A strong solution of this same salt was retained in the mouth for five minutes and then thoroughly ejected; the mouth was carefully rinsed out with fresh water, and wiped dry and again rinsed. As the solution was kept in the anterior portion, and by the tongue prevented from touching the pharynx, there could now have been no possibility of the presence of the salt in the mouth.

The examination of 25 cubic centimetres of urine, passed

two hours and a half afterwards, indicated the presence of a large amount of the bromide.

In these and other experiments not here related the bromide was present in the blood of dogs and in the urine of man, to whom the drug had been administered by means of the mucous membrane of the mouth only, and it is thus proved that bromide of potassium can readily be absorbed by this portion of the mucous membrane. There is no doubt but that the absorption is rapidly accomplished. This is shown in these two experiments and by the following—

Exp. III.—Ten grs. in an aqueous solution were introduced by an œsophagean tube into the stomach of a rabbit. Six minutes after, ʒ iss. of blood was taken from the carotid artery, was carefully analyzed* and found to contain, by calculation, from the amount of bromine obtained, three and three-fifths milligrammes (or $\frac{9}{1000}$ of a grain) of bromide of potassium. We supposed the weight of the rabbit to be 6 lbs. or 5250 grains; from an easy calculation, the inference seems reasonable that, if there was as much of this salt in the rest of the blood as in this specimen, we had recovered about one third of the whole quantity given. Considering that in an analysis of thirty-five centigrammes of bromide of potassium which had been previously dissolved in an ounce of urine, one-tenth part was not recovered in the process, we could suppose that more of the salt might have been recovered in the above named experiment than we actually obtained. In six minutes, therefore, a large portion of the drug was absorbed. Compare this with an experiment made upon man—

Exp. IV.—Eight grains of the salt were swallowed, and the mouth carefully rinsed out with water and wiped several times. Five minutes after, ʒ ij. of saliva were collected,

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* To prevent weariness, the details of this analysis will be reserved for another portion of this paper.

which, analyzed qualitatively, was found to contain a large amount of a bromide from the intense brownish yellow tinge, given to bisulphide of carbon by the addition of two or three drops of strong chlorine water. In another experiment a *slight* indication of the presence of a bromide occurred in urine collected twenty minutes after the ingestion of twenty grains.

Exp. V.—Some blood, drawn from the carotid artery of a rabbit ten minutes after the exhibition of 10 grs. by the stomach tube, failed to show the presence of any free bromine. By adding, however, the chlorine water, a bromide was decomposed, liberating the bromine.

Exp. VI.—The body of the experimenter was immersed for fifteen minutes in a warm bath (96° Fah.), containing an ounce and a quarter of bromide of potassium to 20 gallons of water. The urine passed during the night and following morning was retained, and then a portion carefully tested for a bromide. No indication of its presence was discovered. In this case no eruption of the skin occurred, showing that prolonged contact of the drug does not irritate the skin.

Exp. VII.—At another time about ʒ ij. of this salt was dissolved in a foot-tub containing five gallons of water. The feet and ankles were then immersed for 18 minutes, the temperature of the water being kept at about 72° Fah.

The urine passed three hours after showed, by the usual test, that a salt of bromine was present in it. It is needless to state that no bromide had been taken for four weeks previous to this experiment.

Supplementary Exp. (*a*).—Thinking that the results of these two experiments might be doubted, they both were repeated under somewhat different conditions. I asked my friend Dr. ——— to dissolve ℥ x. of br. of pot. in xx. galls. of water at a temperature above 98° F. He informed me that there was not quite enough water to cover his whole

body, which was kept immersed for 12 minutes, the temperature of the solution varying from 108° to 102° F.; he afterwards washed the body with fresh water, and then wiped it dry. He reported that he felt more languid than usual after a hot bath (the temperature of the water was very high), and imagined that he experienced a saline taste half an hour after the bath. I carefully analyzed his urine (℥ v.) passed during 12 hours after the bath, but could not find the *least* trace of a bromide.

I was prevented from performing this last experiment, because I had, two days before, taken a foot-bath of a solution of this drug, followed the next morning by a cold sponge bath containing ℥ ij. of the bromide to a pail of water. ℥ iv. of urine passed during that day gave a very decided reaction of a salt of bromine. Thus by a warm bath no bromide had been eliminated nor absorbed; while after a cold bath its presence in the urine proved its absorption by the skin. This agrees fully with the statement quoted by Dr. Stillé,* that in a warm bath at 96° F. the body exhales, and at a temperature below 80° the body imbibes moisture.

This drug can be absorbed also by the rectum; for, in the following experiment—

Exp. VIII.—A rectal injection of beef tea, containing an ordinary dose of bromide of potassium, was given to a patient and the urine passed during the next 18 hours collected. ℥ ij. of this gave a decided reaction of a salt of bromine. This, and other experiments since undertaken, prove that the rectum will absorb this drug, when dissolved in a vehicle which will prevent its irritating properties from manifesting themselves. When given in warm water by the rectum, a violent irritation is induced, with a tendency to diarrhœa and tenesmus.

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* Mat. Med. and Therapeutics, vol. i. p. 52.

Prop. A.—These and other experiments, not here related, prove that bromide of potassium is absorbed readily and rapidly by the mucous membrane, generally, and that it is not readily absorbed by the skin except at a temperature below that of blood heat.

Is bromide of potassium decomposed in the system, and may a chemical transformation explain its method of action ?

Dr. Bill, in a late number of the American Journal of Medical Sciences, proposes that there may be a chemical interchange *in the blood* between chloride of sodium and bromide of potassium, and says that when bromide of potassium meets chloride of sodium, chloride of potassium and bromide of sodium result, that is, outside of the body. He also states, that as there is an excess of chlorides eliminated after the use of this drug, perhaps its action may be explained by there being a diminution of the chlorides in the blood. I have taken considerable trouble to determine the correctness of this theory, and can find no other authority for this chemical reaction, and cannot see how such a theory can be proved by the known methods of chemistry.

His argument is based upon the fact that there is an increase in the amount of chlorides eliminated by the urine after the ingestion of the bromide in doses less than what may endanger life. He tabulates a report of the quantity of chloride of silver obtained from the urine of a person taking daily doses of the bromide; and, also, finds an increase in the amount of potassium eliminated by the kidneys. He does not, according to this table, separate the bromide of silver from the chloride of silver.

Consequently an increased amount of the mixed bromide and chloride of silver (which are thrown down together by nitrate of silver), found in the urine after the use of this drug, proves no more than that the bromides and chlorides are eliminated together in the urine. In some analyses

which were conducted under my supervision by Mr. Wood of the present medical class, it was found that this increase in amount of the nitrate of silver *ppt.* was due to the union of bromine and chlorine with the silver; and the bromine was then separated, leaving, I should judge, only the normal amount of chlorine behind.

In our experiments the proportion of bromine to chlorine eliminated was as 2:1.

Another refutation of this theory might be adduced from the fact, that bromide of sodium *never* produces a physiological action which is similar to that produced by the bromide of potassium.

CHAPTER II.

CHEMICAL PROPERTIES OF BROMIDE OF POTASSIUM.

It would be in place here to mention some of the chemical properties of bromide of potassium, and especially in detail the process by which our investigations were conducted.

This is a very fixed salt, losing no weight by fusion at a red heat. The stronger acids with difficulty liberate the bromine at an ordinary temperature. Potassium has a stronger affinity for chlorine than for bromine, and a stronger affinity for bromine than for iodine. If to a solution of bromide of potassium a drop or two of strong chlorine water be added, the bromine is set free in the liquid, and ether, chloroform, or bisulphide of carbon will absorb the gas, producing a strong brownish red tinge to either. The bisulphide of carbon we found of the most advantage in this process.

This is a very delicate test, M. Rabuteau* having discov-

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* Gazette Hebdomadaire, April 24th, 1868.

ered an absurdly small quantity; the only objections to this method being that the organic matters in the urine interfere with its reaction, and that an alkaline fluid may also prevent the decomposition. To overcome these difficulties, we evaporated the urine to dryness, and then ignited the residue, keeping it at a red heat for about half an hour. Then to the solution a drop or two of nitric acid was added, until blue litmus paper was changed to red. Then the chlorine water was added, drop by drop, until the peculiar reddish color was visible. This color was intensified by the addition of two or three drops of the bisulphide of carbon, which, on agitation, absorbed all the bromine. Too much chlorine forms a white precipitate, chloride of bromine; therefore caution is required in adding the chlorine water. Sometimes in the decomposition of the organic matters the bromine escapes, so that the addition of a small fragment of pure soda or potassa may be placed in the urine to take up the bromine*. M. Rabuteau prefers the soda to potassa, because the former is more easily decomposed by nitric acid and chlorine. This was our test for the presence of a bromide.

The process for calculating the amount of a bromide present may be best described by giving a detailed account of *one* experiment—

Exp. IX.—Thirty-five centigrammes of bromide of potassium was dissolved in an ounce (21 grammes) of urine. The urine was evaporated to dryness, charred and ignited. The residue was then treated with boiling water and filtered from the carbon. But little of the coloring matter of the urine was present in the filtrate. Nitrate of silver was then added in excess, and the mixture allowed to stand twenty-four hours. The precipitate was washed thoroughly with

* Op. cit.

boiling water, acidulated with a few drops of nitric acid. This precipitate consisted of a mixture of the bromide and chloride of silver, with some of the coloring matters of the urine. To free the latter, an excess of ammonia was added to dissolve the mixed bromide and chloride, leaving the coloring matters behind. The residue was washed with water. The filtrate was acidulated with nitric acid, and bromide and chloride of silver again precipitated. This precipitate was then washed with acidulated water as before. The filter paper had previously been dried and weight ascertained. This, after being dried, was now weighed with its precipitate and found to have increased to the amount of 6.075 grammes. Of this a portion was placed in a crucible, the weight of which had been previously ascertained.

The weight of crucible without contents was	6.705 grms.
The weight of crucible with contents was	7.025 grms.
Thus the bromide and chloride of silver weighed	.320 grms.

This portion was then fused in the crucible, and chlorine gas, washed in sulphuric acid, was passed over by means of a porcelain tube fitting into the platinum cover of the crucible. The chlorine displaces the bromine which is set free (this process must be continued till the crucible ceases to lose weight). From the loss in weight the amount of bromine may be calculated from the following proportion:*

1. "The difference between the equivalents of chlorine and bromine : the equivalent of bromine = the loss of weight : $\times$." Thus $44.54 : 80 =$ loss of weight : $\times$.

From the amount of bromine originally in the crucible may be calculated the amount of bromine in the whole precipitate as follows:

* Quantitative Chem. Anal. Fresenius, by Bullock & Vacher, p. 446.

2. Weight of the whole mixed precipitate : weight of that in the crucible = × (bromine in the whole) : bromine in the crucible.

From the amount of bromine in the whole mixed precipitate, the amount of bromide of potassium recovered may be determined thus :

3. Equivalent of bromine : equivalent of bromide of potassium = weight of bromine found in the mixed precipitate : × (or bromide of potash).

From these three problems the result of the preceding experiment was obtained. It was supposed that the bromine discovered in this process was united with potassium which was found in the urine. The method of obtaining the amount of this is so difficult and protracted that we are contented with Dr. Bill's* statement, that in his experiments the potash was increased " fourfold " in the urine, when bromide of potassium was taken.

The result obtained was as follows :

Weight of whole mixed precipitate, .	= 0.607	grms.
" " crucible with contents, .	= 7.025	"
" " " without " . .	= 6.705	"
" " bromide and chloride of silver	= 0.320	"
First weighing after passing over the chlorine	= 7.020	"
" loss of weight, ...	= 0.005	"
Second weighing after continuation of same process, ...	= 7.012	"
" loss of weight, . . .	= .013	"
Third weighing, ...	= 6.991	"
" loss, ...	= .034	"

* Quantitative Chem. Anal. Fresenius, by Bullock & Vacher, p. 446.

Fourth weighing, ...	=	6.965 grms.
" loss, ...	=	.060 "
Fifth weighing, ...	=	7.025 "
" loss, ...	=	.062 "

Sixth weight and loss the same. Then using the rules of proportion above stated—

44.54 : 80 = .060 : × = .111 grm. or bromine in crucible.

320:607 = .111 : × = .211 grm. or bromine in the whole amount of precipitate.

80:119.11 = .211 : × = .314 grm. bromide of potassium.

Thus of .350 of a gramme dissolved in urine
.314 " " was recovered.

.036 lost by impure chemicals and an insufficient laboratory.

This was the result of our first quantitative analysis for bromide of potassium, and the process is given in detail to show that all ordinary caution was exercised that could occur to our minds.*

The details of the analyses to be hereafter mentioned will not be transcribed.

M. Rabuteau mentions in a recent publication,† that having tested the urine for two months, and having found a salt of bromine each time, and this when only one gramme had been given, was so much surprised, that he obtained some urine from a person who had not been taking a salt of bromine, and still found a trace of bromine present by the qualitative test before mentioned. In seeking for an explana-

* In these analyses the ordinary commercial nitrate of silver (lunar caustic) was used, which may be the cause of our not obtaining more accurate results. This would not, however, interfere with the *relative* results, which are the main points to be considered.

† Gazette Hebdomadaire, 1868.

tion of this phenomenon he ascertained, if a quantity of urine exceeding one hundred and fifty grammes (℥ vj.+)* was employed he almost invariably detected some bromine. In any less quantity no bromine was perceptible in ordinary urine. Therefore he is disposed to add this metalloid to Bernard's list† of fourteen simple bodies found in man and the higher order of animals.

CHAPTER III.

EFFECTS UPON THE SECRETIONS.

We will now consider the effects of bromide of potassium upon the secretions. The quantity of the saliva does not appear to be modified in any degrée. This drug is absorbed to a very great extent by this secretion, and can be detected for twenty days‡ after one gramme has been taken. Several times have we detected its presence in the saliva within a very few minutes after its administration, and have proved its presence for a long time afterwards, almost as long as it is present in the urine. Voisin states that this drug appears very early in the saliva, and remains there as long as it can be detected in the urine. His statements are confirmed by some experiments of Rabuteau, before alluded to; and, as I have not found any cause to doubt these observers, the details of my experiments have not been given. There seems to be no chemical decomposition with the gastric juice.

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* We never employed in our analyses more than one hundred grammes at any one time.

† Cl. Bernard—Sur les substances toxique et medicamenteuses. Paris. 1867, Page 40.

‡ Gazette Hebdomadaire, 1868, p. 582.

When pure, it at first *may* stimulate the mucous membrane to throw out its mucus; but, after one or two days use, its tendency is to dry up this secretion, mouth and fauces, and especially by the excreta of the intestinal canal, which, with a few exceptions, are dry, hard, and infrequent.

Excretions.—With regard to its effect upon the kidneys there is much debate. Does it or does it not produce diuresis? It is very difficult to judge of this effect, from various reasons, which are evident to every one. The quantity of urine passed in twenty-four hours varies in different persons, and in the same person at different times. The state of the weather, of the skin, of the general health, diarrhœa, constipation, quantity and fluidity of blood, all show their effect upon the urinary secretion, as is shown by a dryness of the mouth and fauces, and especially by the excreta of the intestinal canal, which, with a few exceptions, are dry, hard, and infrequent. Therefore, it is almost impossible to place an individual constantly in the same relations. This may explain some of the inconsistent results determined by various experimenters.*

If it does augment this excretion, it is probably due to the change of blood tension in the kidneys, on which Bernard has found that the activity of kidney secretion in part depends.†

Its effect upon the intestine, it has been stated, seems to be rather constipating, and may be thus explained: by a diminished secretion from the mucous surface, and by a diminution of the reflex sensibility and of the muscular contractility. This is shown by its effect upon the pharynx and all the external portions of the mucous membrane. That this effect is not caused by the immediate contact of the drug

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* Gazette Hebdomadaire, 1868, p. 582. Damourette and Pelvet, Bull. Gen. de Ther., vol. lxiii. p. 296.

† Liquides de l'Organisme, Baillière et fils, t. 2, p. 155.

may be known from the fact that the injection of a weak or a strong solution of this salt into the rectum will create a violent irritation of, and discharge from, the bowel.

I know of two cases of chronic constipation relieved by a dose of the bromide, and where have been taken large doses of the ordinary cathartics without producing an intestinal discharge; in other instances one or two doses of the bromide have caused an evacuation of the bowels. These fæces, carefully and repeatedly analyzed, give no indication of the presence of a bromide.

In such cases the effect of the drug seems to be exerted upon the muscular fibres, inducing their contraction and thus causing an excessive peristalsis, from which there results, sometimes, a violent expulsion of the fæces.

Other cases have been *recorded* where this hypercatharsis was so marked, that the drug was discontinued.

Out of thirty-seven cases treated for epilepsy by this drug,* two patients had to discontinue its use, because catharsis was produced. These cases are, however, very rare, and we are disposed to place them all under the same conditions.

The excretion from the pulmonary mucous membrane, after the continued use of this drug (that is, when the system is under its influence), is diminished, and if the influence is maintained, a dry and annoying cough may be induced.† Hoarseness, aphonia, dry cough, laryngeal pain, sub-crepitant râles,‡ all point to this effect.

The difficulty of expiration,§ oppression, &c., would tend to show a loss of muscular contractility in the pulmonary tissue. If, however, the drug is impure, as it is sometimes combined with the iodide of potassium, opposite results may

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* Williams, abstract in Boston Med. and Surg. Journal, lxxi. p. 422.
† Haneau, Gaz. Heb., 1868.
‡ Voisin, Bull. Generale de Therapeutique, lxxi. p. 101.
§ Ibid. p. 102.

take place, that is, a catarrhal affection of the mucous surfaces.

That bromide of potassium often has iodide mixed with it, has been noticed by Dr. Garrod* some twenty years ago, and there are a few American preparations which also contain this impurity at the present time.

CHAPTER IV.

ELIMINATION.

We will next consider the elimination of this drug from the system, its ways and conditions, before viewing its action upon the economy. Voisin† has considered that, as the breath smells strongly of bromine after the continued use of bromide of potassium, it may be partially eliminated in this way. If this is a fact, then this salt must be decomposed, and the bromine, being volatile, may escape. We find a bromide in the saliva, urine, and sweat, and in each of these a large amount of potash.

Exp. X.—We find that three different persons exhaling for fifteen minutes, after a continued use of bromide of potassium, through glass into a test tube filled with water and bisulphide of carbon, do not produce the yellow color of bromine. On adding a few drops of strong chlorine water, the bisulphide does not change its color. Therefore, we conclude that bromide of potassium is not decomposed nor eliminated by the breath. The peculiar smell this eminent experimenter distinguished, we think no other than that produced by other salts of potassium, such as the chlorate of

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* Medical Times and Gazette.

† Op. cit.

potassa and the iodide of potassium. As this drug, passing through the mouth, mingles with the saliva, and the mucus from the mouth, pharynx, and nose, there could not be any use in analyzing the excretions from the mucous surface of the lungs.

I do not consider that the saliva assists in the elimination of any drug, as, unless accidentally expelled, it passes into the stomach and is again absorbed into the economy. I have already mentioned the ease and rapidity of the absorption of this drug by this secretion.

That bromide of potassium is expelled with the urine has been noticed by Voisin, Damourette and Pelvet, in the Bulletin Générale de Thérapeutique, and by many other observers. My experiments and observations have been made with the particular view of determining under what conditions this occurs, and if certain conclusions be here mentioned which are not altogether new, it may be pardoned, from the fact that the results arrived at are independent of others, because the before mentioned chemical process* has not been carried out by any of the above writers. Great care and much time have been devoted to this subject, and the results, it is hoped, may be of practical value.

It has been already stated that M. Rabuteau has found bromine in normal urine. This, however, only could be found in a quantity much exceeding one hundred and fifty grammes. It would be proper to state that at no time did we use so large a quantity as this in our analysis. This observer states that he has found for twenty days traces of a bromide in the urine of a person who had taken one gramme (grs. xv.) only of this salt, and that traces could also be found in the saliva of the same person during the same period.

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* Vide p. 34.

In our experiments we could only find traces at the end of forty-eight to fifty-two hours after a single dose. If, however, the dose was continued for a few days, the presence of a bromide was apparent for a much longer time, varying with the amount taken and the time the exhibition of the drug was continued. The results of the experiments of M. Voisin* must be doubted, if M. Rabuteau is correct; his idea being that there is a small quantity of bromine present in normal urine; for in their chemical analysis, 325, 400 and 1,000 grammes of urine were used; and the quantity of pure bromide of potassium crystals varied considerably in each analysis. From the 400 grammes, .40 were obtained; from 1,000 grammes, .095; and from 850 grammes, 3.75 grammes. Their method of analysis is not related.

We always, during the first forty hours after an ordinary dose, found distinct signs of the presence of a bromide. The experiments were repeated very often and the same result was obtained.

Exp. XI.—During twenty hours, fifty grains of bromide of potassium were taken in five different doses. The urine passed during the first twenty-four hours was preserved, and amounted to fifty-one ounces. Of this about two ounces was analyzed for the quantity of bromide eliminated by the kidneys; from this amount a little more than one grain, and, by calculation, from the whole fifty-one ounces $28\frac{72}{100}$ grains were recovered. Thus more than one half the amount of bromide of potassium was eliminated during twenty-four hours after the first dose was taken.

Another experiment was undertaken for the purpose of finding how much of the drug was eliminated during the second twenty-four hours after a dose of the drug whose action we are considering.

* Op cit.

Exp. XII.—Ten grains were taken, and, of the thirty ounces of urine passed during the second twenty-four hours, two ounces were carefully analyzed, and, by our process and calculations before mentioned, there was recovered about three and three quarters grains. Thus a third of this salt is eliminated during the second twenty-four hours.

We think that these, combined with other experiments for qualitative analyses for a bromide, would show, that—

Prop. B.—Bromide of potassium is largely and mainly eliminated with the urine by the kidneys, and that during the first day the largest quantity is eliminated, and less during the second day, and so on till there is none left in the system.

Several times were the fæces analyzed and tested for the presence of a bromide, but always with a negative result. A large amount of caustic soda was added before each analysis to allow any free bromine, which might have been volatilized during the ignition of organic matters, to combine in the formation of bromide of sodium. We are forced to conclude either that our chemical process in itself was deficient, or that bromide of potassium is not eliminated with the fæces. In some subsequent investigations for the action of bromide of ammonium, the fæces were carefully collected and analyzed by two members of my class, and no trace of a bromide could be detected. Considering that so large an amount of the salt is eliminated by other organs and the easy absorption by the mucous membrane, it is fair to suppose that, ordinarily, the bromide of potassium, when given in small doses, does not pass through the intestinal canal, but is absorbed before it can mingle with the effete contents of the bowels.

M. Voisin considered that because an eruption of the skin occurred after the continued use of this drug, this organ assisted in its elimination. Acne may be produced from the action of this drug; but why may it not be caused by the

altered condition of the capillary circulation, and thus inducing those inflammatory conditions of the skin, due to an obstruction of the circulation? Indeed M. Hardy in his lectures (sur les maladies cutanées accidentelles) considers this condition of the circulation an important and often neglected cause of acne. As we shall endeavor to show hereafter, bromide of potassium does produce this very effect upon the capillary circulation. To show that this salt is eliminated by the skin, the following experiment was tried, and repeated twice with a similar result.

Exp. XIII.—In the first, forty grains of the drug were taken in two doses three hours apart. Immediately after the second dose I entered a hot-air, commonly called a Turkish, bath, and remained in it one hour, and during that time collected four ounces and a half of perspiration. By a careful analysis, there was found contained a little more than one third of a grain of bromide of potassium in this amount of sweat. It may be remarked that this amount of excretion from the skin was abnormal; but, by the researches of Valentin, the daily amount of sweat is about $1\frac{3}{4}$ pounds, or ℥ xlii. According to approximate calculation, about three grs. might have been eliminated in twenty-four hours through the skin.

This, confirmed by repetition at other times, proves that the skin assists in the elimination of this drug; and moreover we are able by—

Exp. XIV.—To determine whether during the *second* day the elimination by the skin continues; five ounces of sweat were collected in a hot-air bath, entered thirty hours after a dose of eighteen grs. of bromide of potassium. This sweat, treated in the usual manner, showed the presence of a large amount of a bromide.

We may then conclude that—

PROP. C.—The skin assists in the elimination of this drug from the system during the second day as well as the first.

Summary of the means of elimination:—

Of the various organs which carry off the effete matters of the human economy, two only eliminate this drug, viz., the skin and urine. In the exhalations from the lungs and the contents of the rectum, we cannot find any evidence of this drug.

CHAPTER V.

EFFECTS ON THE BLOODVESSELS.

WHEN applied in a state of solution to the interdigital membrane of one posterior extremity of a frog, the web of whose other foot is observed through a microscope, the circulation in the arterioles is seen to be hurried, and the venules become filled with blood of a lighter shade than is generally observed. Soon, the circulation grows slower in the arterioles and the calibre of these vessels diminishes, and the whole capillary system loses its supply of blood, whilst there is less than before in the venules and arterioles. This is probably the result of stimulation, which can otherwise be determined by the muscular contractions observed throughout the trunk and limbs.

Half an hour or more after this, when the animal has become calm, the blood returns to the capillary system in rather larger amount than before; and in a little while the constriction or tetanus of the arterioles is noticed, which continues some time. The blood in the venules diminishes and approaches more to the color of that in the arterioles.* If

* While making these investigations, I attempted some observations on the circulation in the crania of a frog. During the manipulation some bloodvessels

the muscular tissue be now observed without the aid of a lens, it will be found to be pale and exsanguine, which is due to this modification in the supply of blood to the capillary system. This same pallor of skin has been noticed by Voisin in his patients who have been for some time under the influence of bromide of potassium. Can it be that this drug affects the nerves in somewhat the same way that has been observed in chlorosis, interfering with assimilation, and depriving the blood of its proper allowance of red corpuscles? Fortunately, if this theory is correct, the interference is only a temporary one, for the nerves soon after must recover their power, or some serious constitutional trouble would ensue.

Meuriot explains the action of this drug upon the circulation, as compared with atropine, in the following words, which I translate: "It is seen that bromide of potassium acts much more energetically upon the contractility of the vessels than atropine. * * * * The bromide of potassium exaggerates the arterial tonicity, tetanizes the arterioles, slackens or arrests the circulation, and produces an oligæmia of the tissues."*

Some experiments have been undertaken in Berlin, by which it has been proved in a satisfactory manner that upon frogs the sedative action of this drug is upon the vaso-motory nervous system, a translation of which I will give you:

Two frogs were selected, as nearly as possible of the same

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were severed, and the field of vision became afloat with blood corpuscles. The addition of the solution of bromide of potassium changed their color to a peculiar rose red. This occasioned some surprise, but was supposed as something, perhaps, accidental, though unaccountable. However, on reading a memoir by M. Meuriot, I find this same observation with regard to the color produced by the bromide of potassium. This bright red color is probably due to an excess of arterial over venous blood in the capillary system; and as the tissue around is pale, this color is remarkable.

* L'Etude de la Belladonne, Paris, 1868, p. 50.

size; one was used to correct the experiment. The other was poisoned.

FROG NOT POISONED.

The metronome was regulated to 100 vibrations per minute. The legs of the animal were loosely bound together in the middle with soft woollen cords, which would not compress the limbs. A vessel of a given size and with an estimated quantity of distilled water was placed under the feet; then quickly and, at once, all six toes of both feet were cut off at the same height. The time, during which the observation was carried on, was two minutes, or 200 vibrations of the metronome.

The blood, flowing by drops, from each foot was counted, and received into the vessel. (The binding together of the legs being easy had no influence on the value of the *experiment.*)

Number of drops falling into the vessel:.

Right leg.		*Left leg.*
1		2
1		1
1		1
1	in two minutes.	1
2		2
1		1
1		1
8		9

FROG POISONED.

(This frog before poisoning drew his foot out of the sulphuric acid mixture, after nine beats of the metronome).

Waiting till the reflex power in the left leg had sunk to thirty, and in the right to forty-five beats; then all six toes of both feet were cut off at the same level.

In two minutes only two drops exuded from the right leg.

The left plexus ischiadicus was then divided.

In two minutes ten drops of blood flowed from the left leg.

This is a very valuable experiment, the results of which were confirmed by repetition; and shows conclusively, in another way, that the action of this drug is through the vasomotor nerves upon the bloodvessels.

CHAPTER VI.

EFFECTS ON THE NERVOUS SYSTEM.

It might be supposed, from what has been said, that the nerve cells have been impaired by the action of this drug, and that the conductibility of nervous impressions has been interfered with. Nerves from animals poisoned by this drug will convey an electrical current, and electricity will pass through a strong solution, or even through the solid salts of bromide of potassium, isolated in a glass tube, without any diminution of the electric current. The retardation of the circulation must have some dependence upon the reflex nervous system, and we shall soon see that this drug has a decided effect upon this system.

M. Laborde* made some experiments to determine this question, which are well worthy of examination.

He, at first, causes a frog to absorb by the interdigital membrane three centigrammes of this drug. The animal immediately moves spontaneously after the poison is absorbed. Then he remains quiet, but withdraws the extremity if irritated by pinching, pricking, or galvanism. This response ceases in twenty minutes, in the posterior extremities first, and then in the anterior. In another experiment with the same dose the heart pulsates after cessation of responsive action (for two hours). In several of these experiments with this same dose, he reports that a state of tetanism occurred in eight to ten minutes after the absorption of the poison, lasting two minutes, and was then succeeded by a collapse. Also that at first the muscular fibres contract, afterwards are in a state of relaxation, and will not contract by stimulation.

* Archives de Physiologie, 1868, p. 422.

Now these phenomena all point to the same effect as that noticed in the circulation: overstimulation of the power of contractility, soon followed by a state of relaxation or collapse.

To what may this be due? To the direct contact of the salt with the tissue? We have seen that almost immediately a large portion of this drug is absorbed and carried by the blood through the various organs and tissues. We have seen no cause to suppose the decomposition of this salt, but that it is bromide of potassium in the blood, and in the urine and the sweat.

An experiment was suggested to me by this thought.

Exp. XV.—Both of the sciatic nerves of a frog were exposed and were isolated by glass rods. Galvanism caused an equal amount of contraction in both limbs. A sponge was then saturated with distilled water and placed on the right sciatic nerve, and another sponge was saturated with a solution of bromide of potassium (ʒ j. = grs. xiij.) and placed on the left nerve. The galvanic current was then applied to each in turn. The right foot responded to the stimulation. The left foot did not respond to the stimulation. The left was then thoroughly washed with distilled water from a wash-bottle, and then the galvanism applied. The left foot now responded; the bromide sponge was again applied, and the response ceased. The nerve and tissues were again washed and the bromide sponge was applied to the right nerve, the water sponge to the left. The left foot contracted by the stimulation. The right foot did not contract by the stimulation.

This experiment was repeated several times with a similar result, the bromide sponge always preventing transmissibility of the shock through the nerve, while the other, water sponge, did not. These sponges were then applied to the brachial plexus of each side, the nerves being isolated on

glass rods. The poles of the battery were applied one on the nerve, the other on the extremity. The same result followed the application.

This experiment did not hold good with other animals, such as dogs and rabbits, in all of which the transmission of electric current was not prevented by the saturated solution of the drug. An explanation is suggested that the irritation of the drug may in some way have caused a partial anæsthesia of the nerve operated upon.

M. Pelvet,* in speaking of this drug, says that it successively attacks the properties of the sensitive and motor nerves, the brain, cord, medulla, and the muscles. The contractility of the heart outlives every other organ. Respiration is indirectly affected.

Eulenberg and Guttman† say that two to four grammes (ʒ ss. — ʒ j.) injected hypodermically into rabbits kill them in ten minutes, with signs of paralysis of the heart. Internal administration had the same effect. Sensibility and the power of voluntary movement were diminished. They observed always a corrosion of the mucous membrane of the stomach and infiltration of blood. In smaller doses they noticed quiverings in the muscles.

They considered that this acted like other salts of potassium, and presented nothing characteristic of bromine. Pure bromine injected in much larger quantities had no such effects, and did not cause death.

On the contrary, M. Laborde considers that bromide of potassium is the only drug that produces this peculiar action. Bromide of sodium and potassium had an entirely different effect, even when the latter caused death.

With this last observer I am more disposed to coincide, for his experiments were made with much care and ingenuity,

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* Gazette Hebdomadaire, Dec. 6th, 1867.

† Idem, July 5th, 1867.

and more clearly illustrate his views. He states that potassium exalts the power of motility. He endeavors to show, by a very interesting experiment, that bromide of potassium does not destroy the volition, but affects the spinal cord and reflex system only.

A frog is decapitated and laid on the table; to another he caused the absorption of twenty-five centigrammes (four grs. or less). The first loses the sense of reflex power by stimulation, and afterwards exhibits the usual symptoms of tetanus, &c. The frog who had been beheaded has shown no loss of reflex action, but is now caused to absorb the same dose of this drug. The absorption occupies a longer time, but when it is accomplished, this animal also loses reflex response to irritation, and is soon in the same condition as the first frog. From this M. Laborde concludes that bromide of potassium produces paralysis of reflex action, and has nothing whatever dependent upon the volition of the animal experimented upon. In Eulenberg and Guttman's experiments, it may be noticed that they supposed paralysis of voluntary movement in warm-blooded animals. This, however, would be difficult to determine from paralysis of the sensitive nerves; and, as M. Laborde* by his experiments on frogs would show, that not only was there voluntary movement in one or two of these experiments, but that, in the two decapitated frogs (in which volition was put in abeyance), the power of reflex action was not lost until the absorption of the drug; and, as we know that this reflex action is very persistent and of long continuance in beheaded frogs, is it not possible that MM. Eulenberg and Guttman confounded the absence of volition with the loss of reflex action?

Prop. D.—The loss of reflex action is due to anæsthesia induced by the deprivation of blood from the peripheral

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* Op. cit. p. 422.

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nervous extremities, and, also, from the central nervous system, this last occurring after the first. Thus we may get loss of sensation first and then paralysis of reflex action. This is not strange if we compare the syncope produced by excessive hæmorrhage, in which there is anæsthesia and loss of reflex action.

That sedation of the heart's action is, in part, caused by the obstruction to the circulation in the smaller arteries and capillaries, from reduction of their calibre, is evident. This is a physiological law laid down by Marey.* But, probably, the same influence which the drug exerts upon the muscular contractility of the arteries, would eventually enervate the cardiac pulsations.

The first effect produced by a moderate dose is acceleration of the pulse, which in an hour is succeeded by a retardation and diminished impulse. Thus, this drug has been recommended in nervous irritability of the heart where there is hypertrophied muscular tissue from disease of the mitral valve.† Eulenberg and Guttman showed its local action upon the heart, which they thought caused paralysis of that organ. In a large dose applied locally this would very naturally happen, but when administered at a distance slowly received into the circulation a less amount would produce the physiological action peculiar to the drug, without producing paralysis of the heart. In large, or very poisonous doses, Laborde noticed that frogs died very rapidly in a state of muscular relaxation. In moderate doses producing the poisonous action more slowly, the period of tetanism of the muscles occurred first, and subsequent relaxation and death, in which the muscles remain relaxed for some time.‡

* De la Circulation du Sang, Paris, p. 307.

† Stillé, Mat. Med. and Therapeutics.

‡ This apparent difference may be understood, if we consider, that an overdose would not allow the bloodvessels to contract and to pass through the suc-

We have seen by direct experiment, that when applied to the muscular substance in solution, or in a solid form, and electricity is applied, the muscular fibres contract both in frogs and warm-blooded animals. Therefore, this drug does not put in abeyance muscular contractility. M. Laborde,* however, observes that the peripheral extremities of a nerve in a frog under the action of this drug, separated from its central portion by a ligature, conveys electricity and produces contractions in the limb; but the central portion (above the ligature), stimulated by electricity, does not produce contractions in this limb. From this he infers that the action of this drug is upon the spinal cord, and not upon the extremities of the nerves; in other words, he would conclude that in a frog, also other animals and even man, bromide of potassium puts in abeyance the reflex functions of the spinal axis, and that afterwards the extremities of the nerves lose their vitality, and, lastly, the muscular fibres their power of contractility. Now following out this theory upon the action of the heart, we would suppose the same phenomena of action, viz.: That the nerve (par vagum perhaps) is paralyzed by the drug in the ordinary sequence, that is, after the nervous centre has lost its vitality, and that the muscular contractility is preserved, keeping up the cardiac pulsations for a longer or shorter time afterwards. This would seem a very plausible method of explanation; but then, we must not lose sight of the fact that Eulenberg and Guttman, in large doses injected near the region of the

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ceeding steps that we have endeavored to point out in the observations on the capillary circulation. In this case, the vessels are paralyzed and the blood becomes stagnant, thus producing congestion where a therapeutical dose produces *oligæmia*. Congestion in the brain would cause the stupidity and torpor which occurs. There is the same harmony of action upon the bloodvessels and the muscular fibre: where we have tetanus of the former we have tetanus of the latter; relaxation of the former, relaxation of the latter.

* Op. cit., p. 439—Exp. VII.

heart, produced paralysis of this organ. Was this caused by enervation of the nerve, commencing at its central portion, or by destruction of the muscular contractility? If both of the pneumogastric nerves are severed, the animal may live a few hours with great impediment of circulation and respiration, and finally die in a state of exhaustion. Taking into consideration this fact, and that muscular contractility persists after death, we should infer that paralysis of the nerves which regulate the heart's action occurred. Now in frogs the power of muscular contractility is very active and persistent, much more so than in any warm-blooded animals.* Laborde's observations were founded on experiments performed on this animal only, and, therefore, must be accepted with due caution in regard to the action of this drug on the warm-blooded animals, and especially on man, in whom the nervous system is arranged with so much more perfection, and so much more widely distributed, and the functions more subdivided. In all the experiments which I have observed on warm-blooded animals the cardiac pulsations ceased within a few minutes after the signs of respiration. Electrical stimulation produced muscular contractions, whether applied to voluntary or involuntary muscles, to nerves either peripheral or central.

Prop. E.—The action of bromide of potassium on the nervous system may be explained by its action on the capillary, arterial or central circulation; it modifies reflex action, by over stimulation and subsequent paralysis of the vaso-motor system, thus producing oligæmia of the tissues and nerve substances, depriving the latter of the vitalizing pro-

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* We know that cardiac nerve-ganglia maintain their vitality and the cardiac pulsations continue even after the heart is separated from the body; and the ventricle will beat even when separated from the auricle and other ventricle *in frogs*. Likewise respiration in frogs is maintained by the skin, as has been before mentioned.

perties of the blood. *There is no alteration of the nerve substance or cells.*

In what other way can be the explanation of the efficacy of this drug in certain forms of epilepsy, accompanied by a capillary injection of brain or spinal axis, or in hysterical epilepsy where the cause is exaggerated reflex sensibility? Bromide of potassium, though the most certain of all remedies to reduce the number of epileptiform convulsions in certain cases, *never* produces a permanent relief. As soon as the remedy is discontinued the convulsions recur, and in the same manner as before.

The efficacy of belladonna may be explained in the same manner, though the action of this drug* on the capillary circulation is perfectly distinct from that of bromide of potassium.

We would repeat, then, that the action of this drug is perfectly explicable, if we take into consideration merely its action upon the circulation, that the modifications of reflex sensibility may be due to the same cause. If the circulation in a limb is temporarily or permanently arrested by disease or ligature, that limb loses reflex action, and, likewise, sensibility. Is it necessary to lay the blame upon the spinal axis? But it may be said that this method of reasoning does not explain the primary excitement caused by this drug. It most certainly does; for we have always an excitement of the circulation both capillary and central, when this drug is first received into the economy, and then a subsequent sedation of the circulation.

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* Vide Boston Medical and Surgical Journal, March 11, 1869.

CHAPTER VII.

CONCLUSIONS.

I.—Bromide of potassium is easily absorbed by the mucous membrane wherever they are placed in contact.

II.—This drug is *easily* absorbed by the skin, provided the water in which it is dissolved is below the temperature of 75°F. If the temperature is above 96°F. it is not absorbed.

III.—The elimination is conducted by the skin and kidneys; as the saliva is a secretion, its presence in this fluid is not a proof of its elimination.

IV.—In therapeutical doses bromide of potassium is not eliminated by the intestines or lungs.

V.—Bromide of potassium passes out of the system without decomposition. As most of the chemical transformation of drugs takes place, according to Bernard, in the laboratory of the kidneys, Dr. Bill's theory in regard to the interchange between chlorine and bromine in the blood, probably, is erroneous. If there is an interchange, it is in the kidneys, or outside of the body (in other words) that the transformation must occur.

VI.—The effects of the drug are produced by its direct action upon the bloodvessels or the vaso-motor system which control the contraction of these vessels, which explanation may account for all the physiological or therapeutical conditions brought about by the exhibition of this drug.

VII.—There is probably no different or opposing action in proportion to the dose administered, which cannot be said of all drugs. The larger the dose the more intense and the longer the action upon the vaso-motor system.

VIII.—Its action upon the general nervous system is secondary and dependent upon that of the vaso-motor nerves.

That it affects certain parts where there may be a determination of blood is not contrary to the known laws of physiology. Lack of healthy resistance to disturbing influences allows the bloodvessels to be dilated, and, consequently, surcharged; the presence of this drug stirs up the opposing influence which contracts these vessels. This influence would be exerted upon the diseased portion of the system more powerfully than upon the healthy portion.

IX.—Bromide of ammonium, in *almost* every respect, has the same action as bromide of potassium. This, I judge, from the results of more than twenty experiments conducted this last winter with a class at my laboratory (which are herewith appended).

Exp. XVI.—A guinea pig.

At 0 0′ 0″.—4.30 grammes (sixty-one grains about) of bromide of potassium in solution, dissolved in ℥ vj. of water, was injected by means of an œsophagean tube into the stomach.

0 5′ 0″.—Respiration 108 and regular, though inclined towards spasmodic; circulation very rapid.

0 15′ 0″.—Temperature (rectal) 34.8′ (C.);* the animal is sluggish, though sensibility is still preserved.

0 20′ 0″—Pulse about 60, and from this time increased in rapidity though it decreased in force.

0 25′ 0″.—Death, preceded for a few minutes by gasping and spasmodic respiration. Heart ceased beating within a very short time of the cessation of respiration.

Autopsy—immediately after death.—Trachea and œsophagus uninjured. The heart contracts by stimulation. The stomach was finely injected on the external surface, though pale on the internal surface, with here and there a few dark-colored (hæmorrhagic) spots, at which the mucous is easily

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* Normal rectal temperature in this animal is thirty-eight (C.).

separated from the submucous cellular tissue. The brain substance and spinal cord were pale. The membranes at base of the brain and around the spinal cord were injected with venous blood.

Exp. XVII.—Forty-five grains of bromide of ammonium in half an ounce of water were injected, in the same manner as above, into the stomach of a guinea pig.

3′.—The rectal temperature was 38° Cent.

5′.—The respiration became jerky—125 to the minute.

10′.—Cardiac pulsations 108. The animal seemed stupid, but sensibility to irritation preserved; unable to walk; lay on its belly, with the legs extended helplessly behind it.

13′.—Gasped for breath; rectal temp. 36.5° Cent. On pinching in the vicinity of the brachial plexus, or the crural, clonic convulsions were produced, speedily becoming tonic, with marked opisthotonos, and in this state the animal—

28′.—Died, and the muscular spasms were relaxed. At the moment of death the fæces and seminal fluid or mucus (a gelatinous cylindrical mass) were ejected.

Autopsy—fifteen minutes after. Heart and muscles contracted to electric stimulation. Stomach, had the same vascular injection as in Exp. XVI. on its external surface. The contents were squeezed out, and the cavity, blown up with air, was dried, varnished, and is still in our possession. The veins of the cerebral membrane were injected, as well as the venous sinuses at the base. Lungs normal, float on water. Heart normal.

Exp. XVIII.— ʒ ij. of bromide of ammonium in ℥ iij. of water were placed in the stomach of a large-sized healthy rabbit.

3′.—Began to show signs of drowsiness, stopping in his jumps and letting his head fall over to one side; sensibility unaffected. Expelled some fæces.

7′.—Pulse 400.

9′.—Falls on to his belly, all extremities extended; sensibility heightened, starts at a sudden sound or touch; head turned to one side.

14′.—Was taken up by the ears, which was followed by clonic convulsions, accompanied with cries; pupil is dilated; lay quiet upon the floor. Cardiac pulsation 80, irregular; spasmodic muscular contractions, of face and forelegs.

23′.—Another cry, followed by convulsions. Sensibility to touch subsided.

28′.—Another slight convulsion, followed by muscular relaxation and death.

Autopsy.—On opening the skull a gush of dark venous blood came out; capillaries not injected and consequently could not be distinguished; brain substance pale, cerebellum as well as the hemispheres; veins at the base of the skull injected; a clot in pia mater of middle lobe of hemispheres; on either side of the choroid plexus were clots, the plexus itself being distended; a clot in left lateral ventricle almost filling it; lungs floated on water; little urine in bladder; none had been injected from commencement of the administration of the drug. Grs. vi.$\frac{3}{10}$ of bromide of ammonium was collected from the contents of the stomach.

Exp. XIX.—A piece of the skull of a rabbit ($\frac{3}{4}$ of an inch x $\frac{1}{2}$ an inch) was carefully removed, exposing parts of both hemispheres. Ten minutes after the operation, grs. x. of the same salt dissolved in water was placed into the stomach through an œsophagean tube. The bloodvessels of the membrane, covering the exposed surface of the brain, were plainly visible to the naked eye or through a lens, and were filled with dark blood.

Seven minutes after, a contraction of the vessels and a shrinking of the brain substance were plainly apparent. Ten minutes after, the color of the blood gradually changed to a peculiar light red (rose red).

45 min. Capillary injection and expansion of the brain substance; animal is quiet, and the capillary injection is less marked. Slight muscular spasms along the muscles of the back. The brain resumes its natural prominence, then becomes slightly shrunk; and the capillary injection entirely disappears, leaving the brain pale. Two drops of a strong solution of the same salt is applied to the brain; a few minutes after, the capillary injection, caused by the application, is succeeded by anæmia, in which the veins are injected, the capillaries empty, and the brain substance expanded. This is soon succeeded by the same effects as noted above, and the brain remains pale and shrunken; in a state of oligæmia, in which the veins as well as the other vessels are diminished in calibre and devoid of blood. The animal was then killed, as there was no time for further observation. The urine showed the presence of bromine.

Exp. XX.—The nerves of a frog's leg were dissected out as high up as the lumbar vertebræ, and soaked in a strong solution of bromide of ammonium; electricity, applied above and below, caused muscular contractions in the limb. The central end of the nerve was cut off at the spinal cord. The nerve of the other leg treated in the same manner caused the same result.

Exp. XXI.—The soft parts and bone of a frog's thighs were cut off, leaving the sciatic nerves untouched. The nerves were immersed in a bath of a saturated solution of bromide of ammonium, the extremities emerging upon one side of the bath and the trunk upon the opposite side. The poles of the electric battery were applied to different parts of the trunk and to the extremities. The current was transmitted, in every case, producing muscular contractions in the trunk and limbs. One nerve was then divided, and the poles were applied upon the trunk and upon the cut nerve in such a way that the fluid conducted though feebly the electric current.

Exp. XXII.—Perspiration collected during a Turkish bath, without any of the drug being taken, gave no sign of a bromide on analysis.

Exp. XXIII.— ℥ iij. of urine collected during twelve hours after a rectal injection of bromide of ammonium, grs. xx. in beef-tea, showed the presence of a bromide.

Exp. XXIV.— ʒ j. of gastric juice containing bromide of ammonium grs. x., treated without caustic soda and then tested, indicated the presence of a bromide.

Exp. XXV.—Grs. x. of bromide of ammonium were given in a piece of meat to a dog with gastric fistula; seven minutes after, ʒ j.+ of the contents of the stomach were drawn off, which, twice carefully analyzed, indicated no trace of bromine.

Exp. XXVI.—Less than a grain of the bromide of ammonium, placed upon the web of a frog's foot, caused, in the other foot, observed under the microscope: 1st, a contraction of the arterioles and venules; 2d, dilatation of the artery only; contraction of the venules persists.

10 minutes after the application—

3d, the arterial pulsations remain about the same in force and frequency; the arteriole is contracted to half its first capacity; current moves more slowly.

Exp. XXVII.—Grs. ij. of bromide of ammonium were placed upon the web of a frog's foot, and the other foot placed under the microscope.

In 20 minutes the arteriole became contracted as above. The pulsations in the arterioles gradually decrease, and the blood from the venules is received into the larger veins, which after a while in turn become emptied, thus producing what is called oligæmia, or exsanguineous tissue. In all these cases observed, when there was struggling the circulation moved more rapidly, and the bloodvessels were filled with more blood.

Exp. XXVIII.—One of the students in my class took at 7, P.M., grs. xl. of bromide of ammonium in ℥j. of water. Went to bed at 10.30, having noticed only a slight excitement of the circulation, flushing of the face, prickling sensation in the skin and tightness at the temples.

At 2.45, P.M., on the next day, took a rectal injection of grs. xl. in some mucilage of starch.

In 45 minutes experienced the same excitement of the circulation as noticed above, besides feeling a slight nervous excitement, such as he has after taking wine.

1 00′.—Pulse 81.

1 10′.—Pulse 80.

1 15′.—Pulse 81.

1 30′.—The nervous and vascular excitement subsided.

2 00′.—Felt as usual; spent the evening in jovialty and felt no sleepiness. At night, pulse 75, slept well, and had a good appetite for breakfast; dejection normal, but less in quantity. Urine, passed twenty-three hours after the last dose, gave distinct indications of the presence of bromine.

Exp. XXIX.—Another of the students took at 10, P.M. grs. xl. of bromide of ammonium in a claret glass of water, on an empty stomach. Pulse, before taking the dose, 80.

15′.—Pulse 88.

30′.—Pulse 80.

40′.—Vascular excitement, and exhilaration as after taking morphine. This gradually decreased, and was lost in a fit of drowsiness.

1 15′.—Found himself nodding, and then retired; respiration was normal; skin cool and moist.

Arose the next morning at half past six, feeling as well and bright as usual. Had a dejection during the day. Towards evening he noticed uneasiness in the bowels, and the next day decided diarrhœa set in, and lasted for twelve hours; no griping or distress was induced, except

that the abdomen felt as if distended with flatus. He never had diarrhœa, and could see no cause for this attack except from the drug.

Exp. XXX.—At 12 o'clock, before retiring to bed, the same experimenter took ammonii bromidi, grs. xx., on a full stomach. Within half an hour, felt as though he had taken a dose of opium, though less excited. Thinks that he fell asleep, without the aid of the drug; dreamed of trying to pass urine into a bottle, but could not do so on account of being constantly in a crowd of men and women.

Took the same quantity, at 5.30, A.M., on an empty stomach; collected the urine before taking the second dose, which on analysis showed the presence of a bromide.

CHAPTER VIII.

EXPLANATION OF THE ACTION OF THE DRUG.

Permit me, in a few words, to lay before you what I consider is the modus operandi of bromide of potassium, the proofs of which I have attempted to set forth in this paper.

When the drug has been received into the circulation, its action is exerted upon that branch of the vaso-motor system, which causes the contraction of the arterial vessels, thus reducing the supply of blood to all tissues, but acting, more especially, upon those which are superabundantly supplied. At first, for a short time, there may be some vascular excitement, caused by the interference with the natural conditions of the system. Within an hour, and especially after the continued use of the drug, the arterial sedation is accomplished, and may last for several hours. The central organ is also quieted, and thus we obtain a diminution of blood in the

nervous centres, as well as elsewhere, modifying the activity of their functions.

Prof. Brown-Séquard has reported some experiments in the 1st Vol. of his Journal de Physiologie, which illustrate many of the symptoms peculiar to large (but not poisonous) doses of bromide of potassium. In these experiments it is well shown that muscular irritability could be excited by the injection of red blood, even after cadaveric rigidity had ceased, and that ligature of the aorta could prevent this muscular irritability in one hour and thirty-six minutes from the time of occlusion of the artery.

I know of no authentic case of poisoning in man from bromide of potassium. I have no doubt that death could be induced, but the size of the dose must be very considerable. That much harm may be done by too indiscriminate a use there can also be no doubt, as probably a continued use of the drug will interfere with the process of assimilation. I consider its use contra-indicated in anæmia or chlorosis. That it is a specific against epilepsy is erroneous. Such cases as proceed from anæmia of the cord or any part of the brain, will be aggravated by its use. When there is congestion there will be benefit. Again, if the dose could be administered only when an attack is anticipated, it would be following out the indications of its physiological action, and in some cases this idea could be prosecuted. I shall hope in some other way to give a description of its toxicological and therapeutical relations.

CHAPTER IX.

ITS EFFECT ON WAKEFULNESS.

In order to have a full appreciation of the benefits to the generality of cases afflicted with that terrible malady, insomnia, it will be necessary to recall the cause, usually acting in such cases. Those of our readers who would prefer a full and clear understanding on the point should refresh their memory with Dr. Hammond's well known book "On Wakefulness."* I shall content myself more with the *results* of his experiments and observations. He distinguishes natural sleep from stupor and coma; in the former the natural condition of the brain is anæmic, and in the latter, venous congestion; in the latter the brain mass expands in volume, in the former it is reduced and occupies a smaller space.

The proofs of this statement he establishes beyond a doubt by experiments on lower animals, whose skull has been trephined, and by observations on certain persons whose brain has accidentally been exposed. He also cites cases where wakefulness and nightly phantasies can only be explained by this condition, and whose state becomes aggravated by the use of opiates. Also those postures, such as the supine, which determine blood to the head, aggravate, while others, such as the erect and sitting postures, ameliorate these conditions.

Again, the ingestion of easily digested food in moderate quantity determines blood from the brain to the alimentary canal, and will thus induce sleep. A slight amount of cold, such as will cause chilliness to the surface of the body and to the extremities, prevents sleep, probably by driving the

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* Published by J. B. Lippincott & Co., Philadelphia.

blood to the brain.* Over-taxation of the intellectual faculties from the same effect will cause insomnia.

On the other hand, extreme cold or an immoderate meal will induce coma, from a nervous congestion of the brain, and want of aeration of the blood in circulation. This distinguished author states, also, that if in the coma produced by opium in dogs, the trachea be opened and fresh air be forced into the lungs and blood, the coma will be relieved; that in the state of chloroformization the brain is reduced in volume, and of etherization the blood becomes very dark colored, and is probably loaded with excrementitious matters, and congestion and expansion of the brain mass. Pressure on the carotids also induces sleep, and, if continued for a long time in the lower animals, will produce convulsions.

Dr. Hammond, also, states that in the dogs whose skull was trephined, bromide of potassium produced anæmia and shrinkage of the brain mass.

In cases of insomnia arising from the cerebral congestion above mentioned, great benefit has resulted from the use of bromide of potassium, so that now physicians use this drug to produce sleep and to a large extent. However valuable may be a drug in a certain class of symptoms, no sensible man should prescribe this, except where hygienic and moral treatment will fail to accomplish the same purpose. There is a tendency, it is feared, to use a new medicine, and one so especially efficacious as this, many more times than there may be a necessity for so doing. A physiological effect in the economy may be *forced* by the aid of medicine, which may be induced by gentle and natural treatment. The first should not be tried, except in certain cases evident to every practitioner, till the latter fails in relieving the symptoms. For every beneficial effect produced by physic, there general-

* In warm weather and in warm rooms there is with some people an irresistible impulse to sleep.

ly is some unnatural effect which cannot always be counterbalanced by the good.

We have seen that bromide of potassium is not eliminated very rapidly, and that the longer it is used, the more slowly does it pass out of the economy. Is there not reason, then, to be cautious in regard to its use? One case has been mentioned to us by a physician in this city, where an individual, in an Insane Asylum in our vicinity, died after the continued use of this drug prescribed by one of our most eminent physicians, and it was supposed the man's death was hastened if not actually induced by bromide of potassium.

Since writing the above, several cases have been reported, in the journals of France and of this country, of peculiar anæmic symptoms occurring after the continued use of this drug.

LIST OF BOOKS AND AUTHORS REFERRED TO.

J. V. Laborde. Archives de Physiologie, Vol. V., Part. III., 1868.

Eulenberg & Guttman. Gazette Hebdomadaire, 1867, p. 426.

Damourette & Pelvet. Bulletin Générale de Thérapeutique, t. 73, p 292.

Bowditch, H. P. Graduation Thesis, Boston Medical and Surgical Journal, 1868.

Rabuteau. Gazette Hebdomadaire, April 24, 1868.

J. N. Bill. American Journal of the Medical Sciences, July, 1868.

Williams. Medical Times and Gazette. (Abstract from, in the Boston Medical and Surgical Journal, Vol. LXXI., p. 422.)

Voisin. Bulletin Générale de Therapeutique, LXXI., pp. 106 and 153 et sequitur.

Packard. American Journal of the Medical Sciences, July, 1868.

Hodgkins. Medical Times and Gazette, August 1, 1868.

E. Ireland. Medical Times and Gazette, August 29, 1868.

Johnson, W. O. Boston Medical & Surgical Journal, LXXVII., p. 497.

Stone, A. J. " " " " N. S., Vol. I., p. 53.

Hale, J. S. " " " " " " " 78.

Braman, C. B. " " " " " " " 282.

Burr, D. S. " " " " " " " 383.

Monroe, W. F. New York Medical Journal, Vol. VII., p. 327.

Cersoy. American Journal of the Medical Sciences, 1868, p. 543.

Fresenius. Quantitative Chem. Anal. (Bullock & Vacher), p. 446.

Bucquoy. Bulletin Général de Thérapeutique, LXX., p. 371.

Mesnet. L'Union Médicale, by Boston Medical and Surgical Journal, New Series, Vol. I., p. 144.

Hubbell. Boston Medical and Surgical Journal, LXXVI., p. 426.

Begbie. Braithwaite's Retrospect, Part. LV., p. 267.

Hameau.

Garrod. Medical Times and Gazette.

Meuriot. L'Etude de la Belladonne, Paris, p. 50.

Marey. De la Circulation du Sang.

Stillé. Materia Medica and Therapeutics.

Bernard. Liquides de l'Organisme, Tome II., p. 155.

Bernard. Sur les substances toxiques et medicamenteuses, Paris, 1857, page 40.

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MEDICAL AND SURGICAL JOURNAL OFFICE.

1870.

ON THE PRESERVATION

OF

ANATOMICAL SPECIMENS.

By THOMAS DWIGHT, Jr., M.D.

READ MAY 24, 1870.

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ON THE PRESERVATION OF ANATOMICAL SPECIMENS.

It is but to one particular branch of the great subject of preservation that I have the honor of asking the attention of the Society. It is not the art of retaining the form and features of the dead for the comfort of surviving friends, nor that of keeping a "subject" in condition for use at some future period; but that of preserving dissected preparations so that they may for an indefinite time retain the properties which are their original characteristics.

About a year ago I saw in the *Musée Orfila* in Paris, some admirable preparations of extremities, bearing the label "*Procédé de Brissaud et Lascowski*," in which the muscles, instead of being shrivelled cords, were of natural shape and size, flexible, and, in some cases, quite red. Motion in the joints was almost perfect, and the arteries and nerves bore their original relations to the other structures. The oldest of these dissections was dated 1865. I am told that similar results have been obtained in Italy, but I have seen none either in Germany or Great Britain, and I am not aware that any extensive experiments have been made in this country with any fluid of known ingredients. Having declined Dr. Lascowski's offer to sell me the secret, I have endeavored to emulate his success, and it is the results of these experiments that I lay before the Society. This is not merely a new phase of anatomical coquetry; it is eminently practical in its object—namely, to provide the surgeon with a means of refreshing his memory before an operation, such

as neither plates nor dried preparations can give, to render the professor of anatomy less dependent on the varying supply of material, and to enable the private teacher to show in his own office what, till now, has been the perquisite of the dissecting-room. It is not meant, however, in any way to supersede dissection as a means of instruction, for the student never gains so clear an idea of structures, as when he sees them grow beneath his scalpel.

The following were the ends to be kept in view:—1st, to prevent decomposition. 2d, to retain size, form, flexibility and color. For the first object an antiseptic is necessary, and, in these days, carbolic acid naturally suggests itself, and as a vehicle, glycerine is evidently indicated.

Specimen A.—On the 31st of August last, this leg of a dog was dissected and the following mixture very thoroughly rubbed in:—

Mixt. 1.—	Carbolic Acid	1 part.
	Glycerine	5 parts.

In the course of the next two or three weeks this was re-applied once or twice, and the specimen was kept in a cloth wet with the same. No application has been made since. The color is very dark, but, on holding it to the light, the smaller muscles appear red and translucent. The muscles are of about the normal size and very flexible.

Specimen B.—In hopes that alcohol, from its bleaching properties, might counteract the darkening effect of the other ingredients, I applied, on October 24th, the following mixture to this human leg:—

Mixt. 2.—	Glycerine	7 parts.
	Alcohol	3 "
	Carbolic Acid	2 "

It should be stated that the muscles were originally very

small. They are now rather shrunken and not nearly so flexible as in the dog's leg; but in point of color, pretty satisfactory. In the sole of the foot the whole arrangement of the deeper muscles is shown by drawing the superficial ones to one side. Although better in color it is really inferior to the dog's leg.

Specimen C.—On January 25th, I tried the following variation of the last mixture :—

Mixt. 3.—	Glycerine	10 parts.
	Carbolic Acid	2 "
	Alcohol	1 part.

This was applied to a preparation of the interosseous muscles of the foot. The color was perhaps even better than in the last, but the muscles were hard and inflexible. In order to ascertain whether no weaker preservative would answer the purpose, I tried the following experiments, discarding carbolic acid entirely.

Specimen D.—On January 15th, a hand was treated with *Mixture* 4, made by the addition of more chloride of sodium to glycerine than could be dissolved without the application of heat. On February 15th, this specimen was presented to the Museum of the Medical College. The muscles were rather dark, but very flexible. It is still perfectly preserved, but, I understand, rather stiffer.

Specimen E.—On February 1st, *Mixture* 5 was made by adding an excess of chloride of sodium to a mixture of

Glycerine	6 parts.
Alcohol	1 part.

This was applied to this leg, on which some nerves had been dissected as well as the muscles, the latter of a reddish brown, of almost natural size and very flexible. A few weeks

ago there appeared a tendency to develope adipocere, which was easily corrected by repeated applications of alcohol.

The next experiments were to ascertain if the color would not be improved by the addition of nitrate of potash.

SPECIMEN F.—A hand with both arteries and nerves dissected, preserved by a modification of *Mixture* 4 on February 10th.

Mixt. 6.—An excess of
Chloride of Sodium 2 parts,
Nitrate of Potash 1 part,

was thrown into glycerine. The color is almost black and the flexibility is not all that could be desired, though the shape and size of the muscles are satisfactory. A few weeks ago pure glycerine was applied, and the flexibility improved.

SPECIMEN G.—A corresponding change was made in *Mixture* 5.

Mixt. 7.—An excess of
Chloride of Sodium 2 parts,
Nitrate of Potash 1 part,

was added to

Glycerine 6 parts.
Alcohol 1 part.

On February 25th, this was applied to this preparation of the muscles, arteries and nerves of the forearm. The color, though not red, is better than in either of the three preceding specimens, and in all other respects it is almost perfect. Although no attachments of muscles have been divided, the deeper layers with their arteries and nerves can be demonstrated in their natural relations.

It is to be noticed that in the four last mentioned specimens the advantage in color is with those which were treated

with mixtures containing alcohol, not with those containing nitrate of potash. Specimen G, however, in which it is present, is rather better than E, in which it is not. If too much alcohol be used, it causes shrinking and dryness.

Early in April I experimented with another preservative from which much is to be hoped. By the last purification to which carbolic acid, or phenol, is subjected, another agent called cresol is separated from it, which as an antiseptic is equal to the purified phenol. By using carbolic acid, therefore, before its last purification, we have a cheaper agent and one containing a valuable ingredient, which later is lost.

Specimen H.—On April 5th, *Mixture* 8 was made by adding one part of alcohol to four parts of a 5% solution of phenol and cresol in glycerine. This was applied to this female larynx, with the tongue, trachea, œsophagus, thyroid gland and some muscles attached.

Specimen I.—On April 12th, I applied *Mixture* 9, which is the same as the preceding with the omission of the alcohol, to a similar preparation of a male larynx. Both are now in excellent condition, with the exception of the tongues, which are rather dry and shrunken, owing chiefly to the thickness of the mucous membrane. It is probable that a weaker solution would be preferable.

There is a class of preparations consisting chiefly of bone, but of which membranes and ligaments form an important part. To macerate these is to destroy the softer portions, to varnish is to disguise them. If preserved in spirits little can be seen without removing the specimen, which is disagreeable. For this class, a method is particularly desirable, which will maintain the form and softness of the fibrous parts and will keep the bone clean and free from fat.

Specimen J.—On October 16th, a specimen of a subperiosteal, cancerous tumor of the femur was given me. It was no longer fresh, and the soft cancerous mass was of almost fluid consistency. This was washed away, and from the walls of the cavity thin laminæ of bone were found projecting, forming a delicate honeycomb-like structure. The manner in which the periosteum had enveloped the tumor was clearly shown. These, then, were the points to be brought out. *Mixture* 2 was applied externally and injected into the medullary cavity. The bone is now in most places white, but is reddened round the articular surface. The periosteum is soft and its relation to the tumor very evident.

Specimen K.—On February 15th, I received a part of a vertebral column, showing the more or less complete destruction of the bodies of several vertebræ by an aortic aneurism. This was soaked in water to remove the blood, and *Mixture* 3 was applied. To remove the redness, chloride of sodium was very freely used with considerable effect. It is in good condition, and, together with the previously described specimen is to be seen in the Museum of the Medical College.

With regard to the manner of applying the preservative, we must for a moment consider how putrefaction takes place. If we except the interior of the great cavities of the body, there can be little doubt that it originates in spores from the surrounding media. If there be any appreciable amount of blood in the tissues it must be removed, either by soaking or injecting the blood vessels, according to the nature of the specimen; in either case, but particularly the latter, some antiseptic should be used, so that no spores from the water may be deposited in the midst of the tissues. This being guarded against, the only possible point of attack is from the surface, and into this the preservative is thoroughly

rubbed. The specimen is then wrapped in a cloth wet with the same fluid, and other layers are put around to hinder evaporation. It may be advisable in some cases to reapply the mixture once or twice after a few days interval. Specimen F, shows that in cases of subsequent drying, glycerine may be used with advantage. The development of adipocere which occurred in E, was easily remedied by alcohol. It is to be noticed that this has never happened when either carbolic acid or phenol and cresol had been used, and although the results obtained without either of these agents are very satisfactory, I cannot but think it advisable in most cases to use a mixture containing one of them, as more antiseptic and offensive to insects than the mixtures containing neither.

Whether the color of muscles can be perfectly preserved at the same time that the other desiderata are obtained, is still an open question; a judicious mixture of alcohol to a weak solution of phenol (either with or without cresol) in glycerine, is probably the most promising means.

It is very possible that fine results may be obtained by injecting the preservative fluid into the vessels by a long-continued moderate pressure, by means of compressed air or a column of mercury, and by then applying it externally also. I am not aware that this has been tried for this purpose.

Future experiments will doubtless lead to more perfect results, which we may well hope will be of service in the advancement of anatomy, on which foundation all medical science must rest.

MALIGNANT PUSTULE—CHARBON FEVER.

By SILAS E. STONE, M.D.
OF WALPOLE.

READ MAY 24, 1870.

MALIGNANT PUSTULE—CHARBON FEVER.

Dr. John Ware, in a paper read before this Society in 1831,* says, "Every subject in a science of a nature so peculiar as that of medicine, requires not only careful and repeated observation, but the careful and repeated observations of many individuals, in order to its thorough illustration."

I have therefore complied with the request of your committee, not from a sense of fitness on my part, but because it has been my fortune to have had in my practice an unusual number of cases of the effects of charbon poison, and to show my willingness to contribute my share to the elucidation of what has been in this country a rare, if not an obscure, affection.

Until a comparatively recent time there has been much confusion regarding the nature and characteristics of malignant pustule; and to this, I think, the name generally adopted seems to have contributed not a little. It would be difficult to imagine one more unfortunate, for though the disease is malignant in the sense of being dangerous to life, it is not pustular, and is especially distinguished by the *absence* of pus in its active stages.

Tanner, in his Index of Diseases, calls it Malignant Vesicle; and, as this indicates its nature at its commencement, I know of no objection to its use. The only excuse for the contin-

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* Remarks on the History and Treatment of Delirium Tremens—Med. Comm. Mass. Med. Society, 1831.

uation of the first mentioned name is its so general adoption; but I believe it will be much better for medical science when the term Malignant Vesicle, or the more appropriate one of Charbon, shall be generally adopted. To avoid confusion, however, I shall continue to use the former name in this paper.

Under the head of Charbon are included three forms of diseases, viz.:—Malignant Pustule, Malignant Œdema, and Charbon Fever; all caused by one poison. All these have been seen in various epidemics occurring in 1861, '66, '67, '68, and '69.

Twenty-five cases of these three varieties have come under my observation, but as many of these have been published, I will select only those needed for illustration. All these were met with among persons directly or indirectly connected with an establishment for the manufacture of curled hair, and were operatives, except in two instances. Of these two exceptions, one was a carpenter who worked on one of the buildings, and the other was the wife of one of the workmen and did his washing aud mending.

No cases of a similar kind have occurred in the surrounding population, or in connection with other manufactories in the town. Of the twenty-five cases,

14	were classed as	Malignant Pustule.
10	" " "	Charbon Fever.
1	" " "	Malignant Œdema.

Of Malignant Pustule,	5	died	and	9	recovered.
" Charbon Fever,	8	"	"	2	" "
" Malignant Œdema,	1	"			

From this it will be seen that the disease is a very serious one; the mortality amounting to fifty-six per cent. of the whole. In this connection it is interesting to note that inquiries have not shown a similar condition of things to exist in other manufactories of the same kind in the United

States, although this disease has been noticed in a hair factory in England, and at various places on the continent of Europe.*

I will also mention as having some bearing on this subject, that there have been eight sudden deaths in a few years in a small herd of cows pastured in a field adjoining the factory, and fed from the hay. One of these, which died about the time of one of the epidemics of charbon, was examined and pronounced to be a case of splenic apoplexy. This, when we remember that the charbon disease in cattle is known as "*sang de rate*,"† seems to have some significance. No other similar cases have come to my notice in other parts of the town.

As the exposure was constant, and most of the cases were not seen until the disease was somewhat advanced, it was not possible to compare the length of the incubation with the observations of others. It appears, however, that under favorable circumstances, it may be developed in a few hours, but the more common time is from three to eight days.‡

The first appearance of Malignant Pustule resembles the bite of an insect. This is soon followed by a small serous vesicle, which becomes flaccid, or bursts, and shows the skin beneath converted into an eschar the size of the vesicle. Around this is formed a ring of vesicles similar to the first, and the vitality of the skin beneath is soon destroyed as before. Thus two, three, or more, crops of vesicles contribute to form the eschar, which may vary from three sixteenths of an inch to an inch in diameter; though the former size is much more common. As each set of vesicles bursts, the skin beneath becomes dark brown, or black; and dries, form-

* Lawrence's cases in Chelius's System of Surgery, vol. i. p. 77.

† T. Smith, in Holmes's System of Surgery, vol. iv. p. 789.

‡ A. H. Smith, in the American Journal of Medical Sciences, vol. liii.

L. A. Raimbert, Nouv. Dic. de Méd. et Chirurg, Prac., Art. Charbon, and C. W. Pennock in Am. Jour. Med. Sci., vol. xxxvii.

ing a slightly cupped surface. This is situated on an indurated base; and is surrounded by an extensive doughy and and œdematous swelling. A well defined areola is often seen, and sometimes the swelling is of an erysipelatous character. If the case progresses favorably a line of demarkation will be noticed, usually from the third to the seventh day; and it is subsequent to this, and during the separation of the eschar, when the patient is in comparative safety, that the first pus, if any, is noticed. Frequently, if fomentations are not used, the separation goes on without the formation of any pus, the eschar remaining dry and adherent until the tissues are entirely healed.

If, however, the disease does not progress favorably, there is no distinct line of demarkation formed; the swelling continues to increase. If on the face or neck, the fauces are swollen. If the swelling has been of an erysipelatous character, it loses its bright color, and assumes a dusky or purple hue.

From the commencement the patient does not complain of pain in the part affected; and at times the physician will be obliged to search for himself before he will find the diseased spot. In one instance the œdema preceded the vesicle.

The constitutional symptoms occur as follows:—After one or two days of debility, chills occur, followed by fever and prostration. This is accompanied by frequent yawning and gaping, and a sense of indescribable discomfort and restlessness. The pulse from the outset is generally feeble and rapid. Nausea and vomiting occur early in the disease. After the first two or three days, if the case is severe, the patient suffers from great epigastric oppression and distress, and this is a very marked feature in all forms of the disease.

If the case goes on to a fatal termination, the distress continues with increased severity; the senses, which until now have been clear, are obscured by delirium; the pulse becomes

small, feeble, and rapid; and all the usual symptoms of blood-poisoning set in.

As an illustration of the severer forms of Malignant Pustule, I have selected the following unpublished case:—

May 12, 1869.—I was called to see a boy sixteen years of age, always healthy heretofore. He had been taken ill in the middle of the previous afternoon with prostration and vomiting. I found him in bed; perfectly clear in his mind; with pulse 110, feeble. The left side of his neck was swollen, and a gland at angle of jaw of the same side enlarged—gland slightly tender, but neck not at all so. He appeared somewhat excited. Did not relish food, but was willing to take anything advised. A careful search showed no indication of Malignant Pustule. On the next morning found him with pulse 112. The swelling had increased, and a minute pimple like the bite of an insect was noticed on the left cheek. The left tonsil and fauces were swollen, red, and tender. In the afternoon at four o'clock, the pimple was distinctly a charbon spot. It was umbilicated from the coalescing of the vesicles, and the centre was dark in color. He inclined to sigh, and was decidedly thirsty.

14th.—Pulse 116; sleep broken; no appetite; more thirsty, and some nausea. Ring of vesicles enlarged, and surrounded by a small areola. Swelling, both external and in fauces, decidedly increased.

15th.—Had a good night's sleep; mind clear, pulse 116. Eschar well marked, and about $\frac{3}{8}$ of an inch in diameter, surrounded by flaccid vesicles with turbid contents. The swelling had extended to below both nipples, was reddish though not erysipelatous in appearance, and there was no distinct areola. Towards evening became uneasy, and complained of epigastric distress. About seven o'clock he became delirious, tossing from side to side, with occasional

retching. From this time he gradually sunk, and died at twenty minutes of three the next morning.

In this case the intellect was scarcely affected until the last; and swelling was noticed before the vesicle was formed.

In Charbon Fever a different set of symptoms are met with. The patient has been feeling not as well as usual for some days. After a restless night, with perhaps vomiting and prostration, gradually increasing epigastric distress sets in. The respiration is hurried, with frequent moans and sighs; the countenance dusky and pinched; the pulse almost imperceptible. Soon the extremities become cool. The vital powers fail, and death rapidly ensues; the mind often remaining clear until the last.

Two forms have been met with. In one the disease seems to have principally affected the lungs, and in the other the intestinal canal. Of these the following are illustrations:

Dec. 27th, 1867, I was called to Sarah R., aged 16. She had been at her usual work in the picking and sorting room the day before, though she had not been feeling well for a few days. During the previous night she was restless, and was kept awake by uneasiness or pain in epigastrium. In the morning salts were administered, and were followed by two dejections. The distress continued, however, and I was sent for at 5, P. M., her relatives at that time not feeling any alarm.

I found her in bed, tossing from side to side, frequently throwing up her arms, moaning and sighing. There was no pulse at the wrist. Her hands, feet, and face were cold and damp, and of a dull leaden hue. Her mind was perfectly clear. Her answers were correct, and she even smiled when I first spoke to her. She had no cough, but there was dulness over both backs, and mucous râles were heard throughout the lungs. The distress continued with increas-

ing severity until she gradually became unconscious, and died about half past eight the same evening, three and a half hours after I first saw her, and about twenty-four from the appearance of any well marked symptoms. No autopsy was allowed, but large ecchymoses were observed after death.

In this case the principal symptoms were referable to the lungs; while in the following the intestines appear to have been the seat of the disease.

June 20th, 1868. Mrs. P. came to my office, stating that her boy Charles, aged sixteen years, had been unwell three or four days, and remained at home from his work in the spinning room on that account.

She attributed this to a cold, and in the morning gave him a purgative which had not operated. He had been about the house, and out of doors, up to this time. As he had pain in the bowels she came to consult me, and for this I directed castor oil and hot fomentations. At 2 o'clock on the morning of the 21st, I was called. The pain, which he located about the umbilicus, had continued and increased in severity, though two liquid dejections had occurred. He was in bed, constantly throwing himself from side to side. He did not reply when spoken to, and seemed not to hear. His abdomen was flat on percussion; not distended, but the muscles were rigid. The hands and feet were cool; and there was scarcely any pulse at the wrist. He could swallow, but it was with difficulty. A large serous discharge passed unconsciously, while I was there. He continued to sink, and died at 9 the next morning, seven hours after I first saw him.

The autopsy was made by Dr. Morrill Wyman, 32 hours after death. Rigor mortis well marked. Shoulders, back of neck, head, and ears, purple and uniformly so. Eight or ten ounces of clear yellow serum were removed from abdomi-

nal cavity. In the iliac fossa the peritoneum slightly changed, its polish less than natural, and a few capillaries injected. Ecchymosed patches noticed on ileum and cæcum—most marked within a few inches of the ilio-cæcal valve. The cellular tissue around the head of the colon, kidney, and as high up as the liver, was emphysematous and infiltrated with serum, giving it a peculiar gelatinous appearance. The left kidney was pale. The right kidney on section gave some rather dark grumous blood. The tubuli were distinct, and the capsules easily separated. The liver was pale, firm, and of natural size. The gall-bladder was distended with bile. The spleen appeared slightly roughened with whitish dots of lymph. Pancreas presented nothing noticeable. The ileum, at the junction with the cæcum, involving the ilio-cæcal valve, and embracing almost the whole circumference of the intestine, presented a dark purplish brown appearance, with greyish surface. The dark color extended through all the coats of the intestine. Above this, for the distance of four or five feet, gradually diminishing in intensity, the solitary glands were extremely prominent like imbedded shot, and were noticed both in the cæcum and colon. Peyer's patches were not prominent, and nowhere appeared diseased. The mesenteric glands, leading from the junction of the ilium and cæcum and about three feet up the ilium, were enlarged, from the size of a pea to that of a hickory nut. The larger glands were dark colored externally, quite firm, and, on section, appeared mottled with gray. The smaller ones were pink in different degrees of intensity. The bladder contained four or five ounces of clear urine. Lungs soft and crepitating. There was no appearance of any general inflammation of the peritoneum, the spot in the iliac fossa, and the portion covering the spleen, being the only patches at all altered. The heart was healthy, and contained but one small clot.

This case is extremely interesting, as the post-mortem appearances so closely correspond with the descriptions by Raimbert.

May 13th, 1869. While visiting a case of malignant pustule I was called to see a girl sixteen years of age suffering with toothache. She had been restless; and complained of epigastric distress, but attributed it to loss of sleep. The next day after having taken a cathartic, and being no better, I was again called, and found her suffering from intense headache and photophobia, with great epigastric distress and some vomiting. Respiration was sighing, but there was no cough. There was dulness over both backs, and the respiratory murmur was diminished. Pulse 120 and feeble. After a restless night I found the pulse weaker; the sighing less, but the epigastric distress undiminished. Vomiting still continued. Fourth day, pulse 125, stronger; and she had slept some. Slight cough was now noticed for the first time. The expectoration was scanty, yellow and white, streaked with blood. In the evening she seemed to be failing, and there was well-marked subsultus tendinum, with muttering delirium, fumbling and picking at the bed clothes. During the succeeding night she passed exceedingly offensive flatus, and this continued for several days. On the sixth day she revived somewhat, and slept quietly a short time. The respiration was easier, and there was less sighing and distress. Seventh day, pulse stronger, 120 in frequency. Dulness diminished. Expectoration serous in character. On the tenth day the cough became urgent; the expectoration bloody, two drachms of fresh blood being seen. General dulness diminished. On the twelfth, the respiration was less frequent; the right chest was dull on percussion, and respiratory murmur not audible. Left resonant, and respiration there distinct.

Thirteenth, expectoration pus and blood mixed with mucus. Respiration good over both backs. On the right front, from lower edge of fourth rib to top of the third and one and a half inches from sternum, a spot of great dulness was noticed. Subsequently she complained of pain in right side, where was increased dulness; and finally, a gradual rounding out of the side showed that the pleura was filled. About a month after the first attack she suddenly commenced to expectorate large quantities of pus, amounting to two quarts during one day. The chest contracted, respiration was restored, and at the end of two months she was able to sit up and was decidedly convalescent.

In this case we have a disease resembling in many of its characteristics ordinary pneumonia with subsequent empyæma. As the disease progressed the resemblance increased, while the symptoms at the outset characteristic of charbon disappeared. It will be seen, however, that she was taken with peculiar distress and vomiting, and sighing respiration; and, although there was marked dulness over both backs, no cough was noticed until the fourth day of a severe illness. The dulness over a small spot on right chest, after general dulness had disappeared, was peculiar; and the exceedingly offensive flatus noticed in this case corresponds to the observations of others, though not occurring in other cases under my care.

I would say also, that three members of this Society saw the case, and agreed with me as to the nature of the disease.

But one case of Malignant Œdema was seen, and a report of it will give a better idea of the disease than a general description.

June 11th, 1869. Called to see M. D., a strong healthy man, about thirty-four years of age, formerly somewhat intemperate, but for some time strictly temperate, and free

from discoverable disease of lungs or heart. The left side of his face was swollen, and there was a moderate yellow serous disease from that nostril. He had vomited about a pint of yellow and green mucus. In the evening the swelling was increased, and he had vomited again. He was frequently yawning and gaping without being sleepy.

June 12th. He had passed a comfortable night; but the swelling, which was doughy and œdematous, had extended to below the nipples. Vomiting had continued, and sighing respiration was still noticed. During the day he was occasionally faint. The breathing through the nostrils was obstructed by the swelling.

June 14th. Face, neck and chest continued to swell; but the respiration was easier through nostril. Vomiting continued, and delirium was noticed. Up to this time the pulse had been from 104 to 106.

15th. Yawning, sighing, restlessness and swelling were all increased. During the night he had had four thin discharges, the last two containing blood. 16th. The swelling of chest still increased, and was of a dusky red color. Face less swollen, and covered with a dripping perspiration. He had two dejections, mostly of bloody serum. 17th. Two dejections of the same character as before. He was violently delirious. The left leg was much swollen from ankle to knee. From this time he steadily failed, and died at three o'clock on the morning of the 19th. Blisters appeared on leg; and large ecchymoses were seen on back and shoulders after death. No autopsy allowed.

In this case there was no malignant pustule found, though carefully searched for. The swelling was œdematous throughout, and had no tendency to redness till late in the disease.

That these diseases can be produced by the introduction into the system of a specific animal poison, has been abundantly proved by numerous accidents in man, and by direct

experiments with the lower animals. Various opinions have been held as to whether these diseases are also produced by putrescent matter and by the poison of glanders.* But Davaine has shown† that the virus of charbon and putrescent matter are essentially different. And I can much more readily believe that some of the large and remarkably hard pustules of acute glanders, which also resembles charbon in its severer constitutional symptoms, have been mistaken for the latter disease, rather than that the virus of glanders can produce charbon in either of its varieties. Of the various ways in which these diseases are propagated, the more common are inoculation from diseased animals, post-mortem examination of such as have died, and contact with the hides, hair, or wool, of animals which have had the disease. The cases of malignant pustule which have come under my notice seem to have resulted from the application of the virus to an accidental abrasion, probably by the nails of the patients themselves. The localities of the disease indicate this, as in the fourteen cases of malignant pustule the situation was as follows, viz. :—5 were on the face, 6 on the neck, 1 on the shoulder, 1 on the scalp, and 1 on the arm.

In the cases of malignant œdema and charbon fever it seems probable that the patients must have contracted the disease by inhaling or swallowing the dust separated from the hair in the various processes of picking, sorting, spinning, and other methods of manufacture to which it is subjected.

The experiments of Bouley‡ show that the charbon virus retained its activity to an unusual degree; and, as the hair is imported and often remains on hand a long time, the appearance of the disease in these instances is additional evi-

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* Chelius's System of Surgery, vol. i. p. 77.
† Arch. Gén. de Med., p. 369, vol. i. 1869.
‡ Arch. Gen. de Med., vol. i. 1869, p. 371.

dence to the same effect. Dr. Pennock, in his remarks on four cases of malignant pustule occurring in 1834 and 1835,* quotes Boyer to the effect that this poison is not destroyed by the processes in the manufacture of wool or hair into cloth, or hides into leather; and Wagner concluded † that the charbon virus was not decomposed by any process of cooking.

I have found nothing to confirm this last opinion, but certain facts lead me to suspect that it is incorrect.

M. Devaine having demonstrated bacterids in the blood, and serum in the various forms of charbon, has concluded that they are the essential cause of the disease.‡ Dr. Hodges, in a paper on malignant pustule and bacteridiæ (Bost. Med. and Surg. Jour., Jan. 7, 1869), describes the bacterids as follows: "They consist of straight, stiff, cylindrical, detached filaments, varying in length from four to twelve thousandths of a millimetre, and are extremely slender. The largest present, occasionally one and very rarely two, bends at an obtuse angle. By a high power, traces of a division into segments may be detected. They are absolutely without spontaneous movements. On drying they preserve their form and appearance. Sulphuric acid and caustic potash have no distinctive effect upon them. When the blood is in a state of putrefaction the bacteridiæ disappear entirely. This fact, in the estimation of their discoverer, separates them from the whole category of infusoria, which generate in putrefying matter, even if their development in blood, which is, so to speak, living and without any characteristic odor, is not a distinction."

In four of my own cases microscopical examination of the blood or serum, or both, was made. In one instance the

* Am. Jour. Med. Sciences, Nov. 1836.

† Am. Jour. Med. Sciences, No. xxxiv. 1836, p. 481.

‡ For figure, see Nouveau Dictionnaire de Médecine et de Chirurg., vol. vii.

bacterids were found abundantly; in one, a very few; and in the two remaining instances none were found. I am therefore inclined to the opinion of Bouley that bacterids are not essential to charbon.

In all the cases under observation, so far as could be ascertained, the patient had never suffered from the disease before, and in several instances they were operatives recently employed. This, although not conclusive, would lead one to suspect that one attack gives more or less protection against a second.

In no instance was the disease communicated from one person to another; although in several instances some one slept with the patient, and the cases were very freely handled. This corresponds with the experience of other observers; and Dr. Pennock says that wounds which occur in the dissection of the human subject are not followed by this disease, although it has been communicated by the dissections of hares and wolves as well as cattle.

In relation to treatment, I can say that I have followed no routine, but have adapted it to the individual case. The actual cautery was not used, though proposed in two instances. Concentrated tinct. iodine was used in the early stages of several, and for a time I was inclined to attribute good results to it, but my later experience led me to doubt its efficacy. Strong chlorine solution was used in one instance, but in a large number of the cases no local application was made. In this respect they were treated very differently from the recommendations of most of the European authorities; but Wagner, quoted above, writing of an epidemic which occurred in Saxony, in 1834, comes to this conclusion, viz.: "whether the pustule be excised, cauterized, or not touched, the concomitant fever and inflammation proceed in their course, and the duration of the treatment is in no degree abridged, but experience demonstrates

that violent measures oppose the curative efforts of nature and prolong the malady."

This, written more than thirty years ago, expresses exactly my own views.

Internally, tincture of the muriate of iron, quinine, and hyposulphate of soda, were tried as special remedies, but I could not see any decidedly favorable results from them.

To show how differently special remedies are valued by others, I would refer to an epidemic in New Mexico, reported in 1867, by Dr. A. H. Smith, U. S. A., in which he used large doses of tr. ferri mur., and of which he says, "I attribute the loss of my one fatal case to the want of the remedy."*

My own experience leads me to place the highest value on opium in its various preparations, and alcoholic stimulants, of which I prefer the dilute forms. To these I would add ammonia, and particularly quinine from its admitted good effects in similar conditions in other diseases.

Two of my cases (Nos. 1 and 4, Bost. Med. and Surg. Jour., Feb. 13, 1868) went the entire course of the disease without treatment, internal or external, and recovered. These may have their place as furnishing something towards the natural history of the disease, and may be useful in that way. For it has been said by one who made disease a study the better portion of his life—"In order to determine concerning any disease what influence our remedies actually exert upon it, we must first ascertain what will be the course and termination if suffered to go through its usual series of changes without the interference of art."†

This disease is to be especially diagnosticated from erysipelas, malignant or facial carbuncle, acne, bites of insects,

* Am. Jour. Med. Sciences, vol. liii. p. 395.

† Remarks on Del. Tremens, by Dr. John Ware. Comm. Mass. Med. Soc., 1831. Part 2d, p. 177.

and boils; and internally from ordinary congestion of the lungs, and peritonitis.

It should be remembered, too, that the erysipelatous inflammation which accompanies the local lesion, may so obscure it as to allow it to escape notice. This I have known to occur in some instances.

But probably all other diseases together have not created so much confusion in this respect as malignant carbuncle, otherwise called malignant feruncle or facial carbuncle. This has often been called malignant pustule, though the distinction has long since been pointed out by Bourgeois and others, and it would deserve the name had it not been already appropriated. They resemble each other in often occurring on the face, being accompanied by enormous swelling, and by frequently becoming fatal in a few days. They contrast with each other in that malignant carbuncle resembles an ordinary carbuncle—is conical, red, painful, and purulent; whereas local charbon is slightly depressed, umbilicated, with a dark centre, and is insensible and not purulent.

The diagnosis of individual cases of malignant œdema and charbon fever is admitted to be difficult. Malignant œdema will be likely to be confounded with erysipelas, and charbon fever will more often be mistaken for ordinary congestion of the lungs or pneumonia. Where the intestines are principally invaded, it might be mistaken for peritonitis or perforation.

The intensity of the disease, the rapidity of its progress, and, very probably, the occurrence of some cases of malignant pustule in the same population, will frequently give a clue to the cause, identify the disease, and confirm the diagnosis.

LATE CONTRIBUTIONS

TO

AURAL SURGERY.

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By CLARENCE J. BLAKE, M.D.

OF BOSTON.

READ, MAY 24, 1870.

LATE CONTRIBUTIONS TO AURAL SURGERY.

The principal subject of this paper is one which has, as yet, received comparatively little general attention in this country.

The treatment of diseases of the ear, aside from general practice, has had few advocates, while the study of the subject has hardly kept pace with that of other branches of surgery, and it is only very lately that opportunities for instruction in otology, and for clinical study, have been afforded by our medical schools, and that we have had suitable text books for the use of students. The appointments of qualified lecturers in the New-York and Philadelphia Hospitals, and in the Harvard Medical School, have supplied the former, and the revised edition of Toynbee, and Dr. Roosa's translation of v. Tröltsch, the latter want.

Aural surgery, or the surgical treatment of affections of the ear, considered as a separate branch of the general science, is of very recent growth. Thirty years ago, the knowledge of the pathology and treatment of diseases of the ear was very limited, and to-day it is far behind those other departments of medicine which have received especial attention.

This is the more surprising, as the organ implicated plays so important a part in our daily life, and is the channel through which we receive many of the impressions which form and mould the character. As a special sense, hearing ranks only second to sight in importance, and a loss of it entails to a great degree the loss of everything that serves to make life useful and enjoyable.

The rarity of affections of the ear cannot have been the ground for the neglect of surgical interference in their treatment; this is to be traced rather to the situation of the more important parts of the organ, at the termination of a long narrow canal and in close relation to so vital a structure as the brain; to the delicacy of the functional apparatus of hearing, and to the fact that diseases of the ear are by no means so patent as those of the eye for instance, where a slight functional disturbance excites immediate attention and calls for speedy relief.

Until Toynbee opened the way for a more thorough study by numerous and careful observations upon the pathology of diseases of the ear, the general treatment of this class of affections was for the most part empirical, and the surgery was almost entirely confined to the removal of foreign bodies from the meatus, and operations upon the auricle.*

Contemporaneously with Toynbee, Wilde of Dublin contributed largely towards increasing the knowledge of this subject, by founding an aural clinic and instituting careful examinations of his cases, and so aided in laying the foundations of the future science of otology. The labors of Toynbee and Wilde gave an impetus to scientific investigation on the continent, and in the large universities numerous observers turned their attention to a field which had long been culpably neglected, and which, as they advanced, they found to promise a rich harvest. Nor has this promise been otherwise than fulfilled, for now almost every day brings the news of some further discovery in the anatomy or physiology of the organ of hearing, some further contribution to our knowledge of the pathology of its diseases, or the gratifying assurance that the list of so-called incurable affections

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* The illustrations in most of the older works upon aural practice are confined principally to different forms of the ear trumpet.

has been still further diminished. Up to within a few years, the only methods of examining the ear were by means of a probe, or by placing the patient in such a position that sun-light or the light of a lamp should shine into the meatus. The use of the probe was obviously objectionable, and that the simple illumination employed gave but an imperfect idea of the condition of the membrana tympani is shown in the illustrations of some of the earlier authors.*

Kramer and Itard employed a bi-valve speculum for the purpose of dilating the meatus and concentrating the light upon the membrana tympani; this instrument is still in general use, but is by no means so efficient as the specula of Toynbee, Gruber, v. Tröltsch and Politzer, which are rapidly taking its place. In using Kramer's bi-valve speculum, the vision is obstructed by the hairs which project through the openings between the valves, and, moreover, in the anxiety to obtain a clear view, the observer may forget the delicacy of the integument lining the meatus, and exert a pressure liable to excite inflammation.

Gruber the elder, of Vienna, first employed a simple conical tube of metal in place of Kramer's instrument, and the specula since constructed are merely modifications of this. Politzer has retained the form of the original almost exactly, but has substituted hard rubber for metal, on the grounds of cheapness and lightness; the dark ground throws out the coloring of the membrana tympani more distinctly, and it is not necessary to warm the instrument before introducing it, as is often required in the case of the metal tubes.

It was not till specula had been some time in use that any change was made in the method of illumination.

In 1855, v. Tröltsch devised a concave mirror, for the purpose of reflecting natural or artificial light, and concen-

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* Compare Frank's Ohrenheilkunde, p. 297.

trating it within the speculum. The orifice in the centre of the mirror is for the convenience of the surgeon, the eye being brought in a direct line with the focal point. This mirror, though original with v. Tröltsch, had been suggested by a Westphalian surgeon—Dr. Hoffman—some fourteen years previously, but had not received the attention which it merited.

Still earlier, in 1834 namely, Bonnafont had constructed a tubular reflector to be used with Kramer's speculum. It consisted of a copper tube 20 c.m. long, and 15 m.m. in diameter, containing three lenses. Towards the lower end, and placed diagonally across the tube, was a centrally perforated mirror to receive the light concentrated upon it from a funnel-shaped cylinder, placed at right angles to the tube, and reflect it into the meatus. This instrument of Bonnafont's has served as the model for all of that class of reflectors receiving the light from a direction at a right angle to the long axis of the instrument.

Of the modifications of Bonnafont's reflector, Leiter's and Hinton's are perhaps the best. The one made by Leiter, of Vienna, is very simple in its construction; it is of hard rubber, dispenses with the funnel-shaped reflector, and can be fitted at its lower end with specula of different sizes. The other, designed by Mr. Hinton, of London, is a very elegant instrument; with it two persons are enabled to make an examination at the same time, and it is therefore valuable for purposes of demonstration.

Since the first attempts at obtaining a clear illumination of the membrana tympani, the idea of facilitating the examination of that structure by magnifying has been a favorite one with otologists. The instrument of Mr. Hinton does this to a slight degree, and Weber, of Berlin, has constructed a lens speculum with this object in view. The speculum

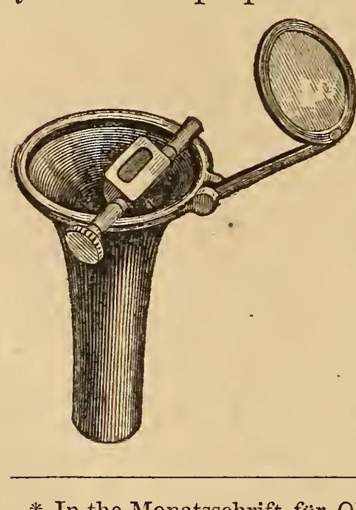
is fastened at one end of a metal frame, and the reflector at the other, the lens being placed just in front of the mouth of the speculum. Aside from the difficulty of adjusting the lens, the instrument, taken as a whole, is clumsy in construction, and requires considerable experience in its use.

All of the tubular reflectors and lens specula serve only for examination, and are of no immediate use when we come to operate upon the membrana tympani or within the middle ear. In this case we may employ either the direct rays of the sun, or artificial light, or the concave mirror attached to a spectacle frame or to a band passing around the head, or the instrument of Mr. Ritchie, to which I wish to draw especial attention.

In all operations within the ear, a clear and steady illumination is of the first importance. Where direct light of any kind is employed, a difficulty presents itself in that the ear to be operated upon, being towards the source from whence the light comes, the operator must so place himself that he may obtain an unobstructed view, and still not cast a shadow upon the field of operation; and this is by no means an easy matter. The concave reflector obviates this difficulty to a certain extent, but not entirely. The light must be reflected from the mirror held in the hand or fixed upon the head in the manner described, and the hand of the operator coming between the mirror and the mouth of the speculum, necessarily casts a shadow. In addition to following the point of his instrument, the surgeon must give a due consideration to the illumination, and keep his head and the mirror steadily in proper position; and, moreover, in order to get the clearest view, it is necessary to look through the orifice in the centre of the mirror, so that only one eye can be used: the natural result is that the stereoscopic effect is lost, and it is very difficult to estimate the distance between the point of the instru-

ment and the eye.* Dr. Edward H. Clarke employs a mirror attached to a stand by a universal joint, for reflecting sunlight from a window into the room, and then by means of a lens of about two inches focus throws the light through a silver speculum into the meatus. By this means the degree of illumination can be very easily regulated, and a strong light can be thrown at will upon any part of the membrana tympani which it is desired to examine.

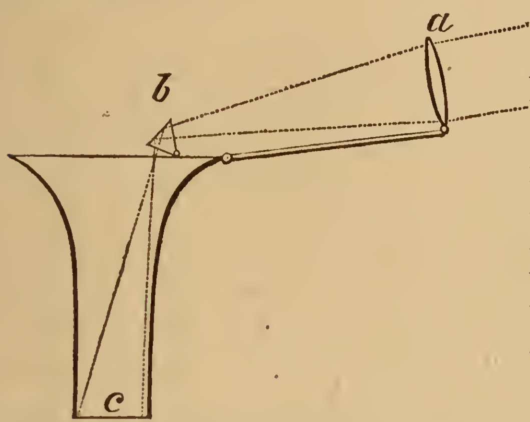
For the sake of dispensing with the reflector, any attempt to bring the light between the operator and the patient would be useless; but to bring the light from one side and reflect it over the rim of the speculum, leaving the operator a clear field, and relieving him of the care of managing the illumination, would greatly facilitate the performance of the more delicate operations upon and within the membrana tympani, especially those requiring time as well as care. On stating the requirements of the case to Mr. Edward S. Ritchie of this city, one of the ablest mechanicians in the country, he devised the instrument to which I have given his name, and which, while exceedingly simple in its construction, perfectly fulfils the purpose for which it was intended.



It consists of a hand rubber speculum (Politzer's) of the largest size, fitted with a metallic rim, to which is attached a revolving prism and an arm, bearing at its outer end a lens of about an inch focus; this arm is moveable, but sufficiently firm to remain fixed at any angle at which it is placed. The prism is just within the focal distance of the lens,

* In the Monatsschrift für Ohrenheilkunde, December, 1869, Dr. DeRossi, of Genoa, under the title "L'Otoscope binoculaire," describes a concave reflector constructed on the principle of the binocular ophthalmoscope.

and its incident face is armed with a small metal shield, having an opening in the centre corresponding in its short diameter to the diameter of the pencil of light falling upon it from the lens.



The advantage of the prism over a mirror or other reflecting surface is, that we have almost total reflection, and but little of the light concentrated upon the prism by the lens is lost.

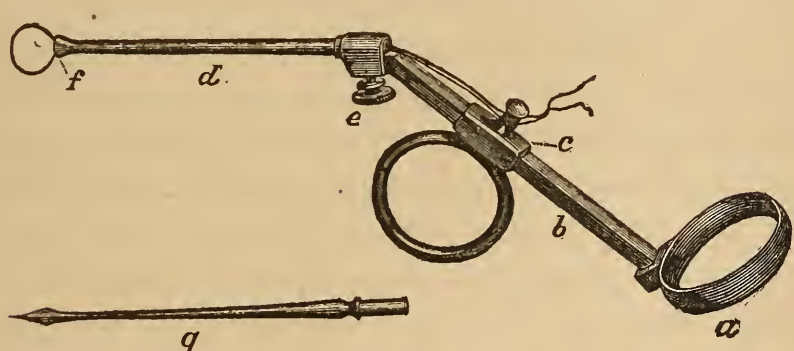
In operating, an assistant is required to draw the auricle upward and backward, and keep the speculum in position, with the pencil of light upon the opening in the shield of the prism. It is not claimed for this instrument that it at all supersedes the head mirror of v. Tröltsch, but it is certainly of great advantage in the more complicated operations, where a steady and uniform illumination is indispensable. The instrument, as a whole, weighs only about one hundred and fifty grains, and can be made much lighter; so that when once firmly inserted in the meatus, it remains in position, and there is no necessity for holding it nor fear of its slipping out of place during the operation. I have had ample opportunity for testing it, and find, as before intimated, that it greatly facilitates the performance of delicate operations.

As a result of inflammation of the middle ear, especially where it has occurred in childhood, we sometimes find adhesion of the lower end of the malleus to the promontory, and also synechiæ extending from the membrana tympani to various points on the wall of the tympanum and to the ossicula, sometimes forming quite a network. These adhe-

sions greatly impair the functional activity of the sound-transmitting apparatus, by tying down the membrana tympani and destroying its vibratory power, and preventing movement of the ossicula. In 1864, Dr. Sigle, of Stuttgart, described an instrument of his own invention, the purpose of which was to facilitate the diagnosis of the presence of synechiæ, implicating the membrana tympani. It consists of a hard rubber speculum, the mouth of which forms a circular chamber, closed at its outer end by a thin plate of glass set at an angle of about 50° to the long axis of the speculum. The chamber has an opening in its floor communicating with a flexible tube having a mouth-piece. The speculum being pressed tightly into the meatus, the air is exhausted by suction through the tube, the air in the middle ear presses the membrana tympani outward, and those points at which there are adhesions are represented by slight depressions upon the general surface. By this means we are enabled not only to make an accurate diagnosis, but also to determine at what point to apply the knife for the purpose of liberating the membrane.

Among the cases of disease of the ear which most frequently apply to the general practitioner for relief, are those forms of inflammation of the middle ear and the meatus, accompanied by a more or less offensive discharge and liable to the complication of aural polypi. Whatever may be the origin of these growths, their extirpation is indispensable to the successful treatment of the affection which they either originate or accompany. Of the means for effecting this purpose, the forceps, wire snare, and caustic applications, are most frequently employed. The forceps should never be used unless we are satisfied as to the point of origin of the polyp, and that none of the important structures of the ear are implicated in the growth. The satisfaction incident to the clean extraction of a large polyp, would be very much

diminished by finding a portion of the membrana tympani or one of the ossicula attached to it. Of wire snares, Sir William Wilde's is the one described in all the later text-books, and most generally employed. In adding one more to the modifications of this instrument, I have endeavored to obviate a difficulty which I had found in its use, and also by a slight change to adapt it to another purpose. In Mr. Wilde's snare, the bar which carries the slide and the arm which supports the wire are in one piece. To slip the wire loop over a small polyp in the meatus when it grows from one of the side walls, it is necessary either to turn the whole instrument or else to twist the wire loop upon itself. The former is inconvenient; in the latter case, where the loop is drawn taut, it tends to return to its original position, and may either slip over the polyp or cut it off obliquely, leaving a larger portion than is desirable for extirpation by the caustic. The wires running upon the outside of the arm are apt to be in the way if slackened, and in a narrow meatus may cut into the integument if drawn upon.



For the fixed arm a moveable tube of German silver (*d*) is substituted. This tube expands at the outer end into a flattened head (*f*), having two openings for the passage of the wire; the inner end of the tube fits into a broad band on the slide bar (*b*). The ends of the wire passing down the tube are fastened to a pin on the upper part of the slide (*c*),

below which is a ring by which traction can be made. The slide is substantially the same as in the snare of Dr. E. H. Clarke.* When ready for use, the metal tube, and with it the loop, can be turned in any desired direction, and firmly fixed in place by means of the small set screw (*e*). By unscrewing the thumb-ring (*a*), removing the slide and the tube and inserting in the place of the latter the lance-headed needle (*g*) employed for performing paracentesis, we have a myringotome possessing the advantage of enabling us to adjust the cutting edges according to the direction in which it is desired to make the incision.

In no one section of aural surgery have such rapid advances been made as in the diagnosis and treatment of affections of the middle ear, and much still remains to be done. A glance at the anatomy of this portion of the organ of hearing shows us the important part which it plays in transmitting the vibrations received through the meatus to the internal ear, and an examination into the delicacy of structure and accurate adaptation of the several parts to the office which they have to perform, shows in what manner a slight lesion may result in extensive impairment of hearing.

Aside from the effects of primary affections of the membrana tympani, the sequelæ of acute and chronic inflammation of the middle ear are manifold, and in a large proportion of the adult cases seeking relief from impairment of hearing and the accompanying subjective symptoms, the existing cause is traceable to structural lesions, the result of an inflammatory process occurring perhaps years previously.

It is in this class of cases, in addition to treatment instituted by means of catheterization by the Eustachian tube, that we most often are obliged to have recourse to direct surgical

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* Observations on the Nature and Treatment of Polypus of the Ear. Boston: 1867. P. 58.

interference through the meatus, with the hope of anything like permanent relief to the patient, and in this direction it would seem that the most important improvements in aural surgery during the next few years are to be made.

The operation of paracentesis of the membrana tympani is by no means of modern origin; its application, however, has of late been very much extended and the limit of cases within which it may be employed to advantage has been more accurately determined. In 1649, Riolan propounded the question as to the advisability of destroying a portion of the membrana tympani in cases of deafness; this proposition was founded upon his observations of a case where the hearing was restored by an ear-spoon having been forced into the ear, tearing the membrane and fracturing the ossicula. The value of Riolan's observations and suggestion was overlooked, however, and intentional laceration of the membrana tympani considered reprehensible. So strong was the feeling on this subject that, in 1750, Cheselden, who had been following out the experiments of Willis and Valsalva upon dogs, was prevented by popular outcry from operating upon a criminal condemned to death.* The credit of the first successful operation has been given to several surgeons, among whom the name of Sir Astley Cooper ranked first, but it is now pretty clearly established that a travelling charlatan named Eli employed it empirically for the cure of deafness in Paris, in 1760, but met with but a moderate degree of success.

In 1801, Cooper followed the publication of his observations upon the effects of perforations of the membrana tympani upon the hearing, by reports of successful cases in which he had operated, propounding closure of the Eustachian tube as the *single* indication for the operation. Subsequent cases did not confirm the hopes raised by the first

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* Schwartze: Archiv für Ohrenheilkunde, Bd. II., Hft. I.

results, and, dreading the effect upon his reputation and discouraged by ill success in a large proportion of the patients who flocked to him from all parts of Europe, he finally abandoned it. His first publication, however, had attracted universal attention upon the continent, innumerable instruments for attaining the desired end were invented, and there was a general and indiscriminate cutting, tearing and trephining of tympanic membranes from one end of Germany to the other.

Himly, of Göttingen, put an end to this reckless experimenting by proving that the operation was by no means either harmless or a universal remedy for deafness, and could only be of service in certain cases, very few in number; and it shortly fell into more disrepute than it deserved. But science was by no means a loser; the haphazard operators became careful observers; the numerous cases operated upon supplied ample material, and the first step was taken towards a knowledge of the functions of the membrana tympani and ossicula, and an appreciation of the importance of the study of their pathology. As a result, the indications for this operation were more clearly determined, and later observers have greatly enlarged the list of cases in which paracentesis may be resorted to as the readiest means of relief. Obstructions of the Eustachian tube, thickening of the membrana tympani, and the accumulation of blood, pus and mucus in the tympanum, were considered the sole indications for the operation by the majority of authors, up to within ten years. Frank, however, in his Lehrbuch der Ohrenheilkunde, 1845, lays particular stress upon its diagnostic value in cases of thickening of the membrane and occlusion of the Eustachian tube as a means of determining the character of the contents of the tympanum.* The employment of bougies for the

* Frank: Praktische Anleitung zur Erkentniss u. Behandlung der Ohrenkrankheiten. Erlangen: 1845. Pp. 74, 75.

relief of obstruction of the Eustachian tube* obviated the necessity for the more serious procedure, and limited it to cases of atresia of that passage; and Gruber, of Vienna, in an article on Myringodectomy, 1863,† contributed greatly to extending its applicability, by giving four indications in addition to those already enumerated, namely—partial ossification (calcareous deposits) of the membrana tympani when accompanied by loss of hearing and subjective noises; cicatrices which interfere with the function of this structure; attenuation of the membrana tympani when in consequence of the loss of its resiliency it is pressed inwards and excites subjective symptoms; abnormal adhesions of the structures within the tympanum, when their character and extent can be accurately determined, and the operation will open the way for their removal.

In the purulent inflammation of the middle ear frequently accompanying scarlet fever or measles, the operation, if resorted to in time, may be the means of warding off the disastrous results which entail a greater or less degree of permanent deafness, and contribute a goodly percentage to the inmates of our deaf-mute institutions. In the natural course of the disease, that is in the severer cases, as a general rule the pressure of the confined discharge upon the inflamed membrana tympani gradually causes thinning and rupture, leaving an opening which enlarges as the inflammation and discharge continue, until the greater portion of the membrane may be destroyed and such changes take place within the tympanum as to result finally in a condition of things which it is beyond the present degree of surgical skill to remedy. A timely use of the knife would relieve the pres-

* Schwartze, l. c.

† Allg. Wiener Med. Zeitung, 1863.

sure upon the membrane and within the tympanum, and by permitting an early escape of the discharge would favor resolution.

Quite recently considerable attention has been drawn to the value of paracentesis in cases of accumulation of excessive mucous secretion within the tympanum, by Dr. Adam Politzer,* Prof. Moos,† Mr. Hinton‡ and others, and their observations show a much greater frequency of such cases than was generally supposed.

The accumulation of the secretion in the tympanum is the result of an inflammation in that cavity, accompanied or followed by diminished permeability of the Eustachian tube: the fluid is often very thick and tenacious, and does not easily make its escape by the natural channel down into the throat; by its pressure upon the membrana tympani it interferes with the functions of the sound-transmitting apparatus and greatly diminishes the hearing power, while a corresponding pressure upon the base of the stapes and the membrane of the fenestra rotunda, gives rise to ringing, singing or rushing sounds, and reflexly causes vertigo, nausea and even vomiting. Catheterization of the Eustachian tube or the use of Politzer's air douche, give but partial relief, the inflammatory process having generally decreased the patency of that passage in a greater or less degree, so that exit is permitted to but a small portion of the fluid, and that which remains serves to keep up the irritation and favor a still further secretion. A free incision through the membrana tympani enables us to force the secretion outwards into the meatus by injecting air through the Eustachian tube, and this procedure

* Wiener Med. Wochenschrift, 1867.

† Archiv. of Ophthalmology and Otology, 1870.

‡ Guy's Hospital Reports, 1869.

is generally followed by an immediate increase in hearing and freedom from the distressing subjective symptoms.*

The opening in the membrane closes readily within three or four days, and it is sometimes necessary to repeat the incision four, five or even six times. Catheterization or the simple air douche should always accompany the operation, and it is well to continue it for some time after the tendency to excessive secretion is diminished, in order to restore the membrane lining the tympanum to a healthy condition, and secure a sufficient permeability of the Eustachian tube to guard against the liability of a recurrence of the trouble. The fact above mentioned concerning the readiness with which incisions through the membrana tympani become closed brings us to the consideration of a question which has long occupied the attention of aural surgeons, namely—the possibility of establishing a permanent opening into the tympanic cavity.

Where the sound-transmitting apparatus no longer fulfils its office, and the membrana tympani remains intact, it presents an obstacle to the passage of sonorous vibrations. In such case, if there is no contra-indication, such as an affection of the labyrinth or of the vestibular attachment of the stapes, an opening through the membrana tympani would allow the sound waves to pass into the tympanum, and, falling upon the base of the stapes, to be transmitted to the auditory nerve. The instruments first employed for paracentesis were either fine trochars, needles, stilets or knives of various shapes; it was soon found that the openings closed completely, and that the relief gained by the operation was only temporary. Trephines were then resorted to, but with no better success, and the excision of even a large portion of the mem-

* Mr. Toynbee proposed the removal of fluid from the tympanum by means of a small syringe introduced through the membrana tympani. Trans. Am. Otological Society, 1869.

brane did not prevent subsequent closure of the opening. Bonnafont followed trephining by cauterization of the edges of the wound and by the introduction of bougies and metallic canulæ, in one case allowing a canula to remain forty-five days; within six days after its removal, however, the size of the opening had so far diminished that it was impossible to reinsert the tube.* Perforations were also made with caustics, but closed with equal rapidity, and this method was found objectionable on account of the danger of exciting severe inflammation.

In 1859, Erhard proposed the substitution of a wire heated by an electric current, for caustic, and the white-hot trochar. Voltolini, who had occupied himself with the application of galvano-caustic in the treatment of diseases of the throat and for the cauterization of aural polypi, developed the idea, and in December, 1867, published an account of his first operation and the instrument employed.†

About a year later I had the opportunity of witnessing in Vienna the experiments of Drs. Politzer and Chimani and Prof. Moos, with galvano-caustic. The instrument was the simple point of fine platinum wire used in the cauterization of granulations and polypi. The object to be attained, a perforation made quickly and painlessly, which should remain open without further interference. Three cases were selected from patients in the aural clinic of the Garrison Hospital No. 1, and submitted to the experiment. The platinum point, being pressed against the membrane in the anterior inferior segment, was quickly brought to a white heat by a battery consisting of two Grove's cells. Instead of passing painlessly through, however, in each case the pain caused was so severe that further attempts were abandoned.

* Bonnafont: Traité théorique et pratique des Maladies de l'Oreille. Paris: 1860. P. 375.

† Monatschrift für Ohrenheilkunde, 1867, No. 3.

Four days later I examined one of the patients and found an acute myringitis on both sides, and it is not improbable that the result was the same in the two other cases, judging from the fact that the pain and congestion following the application of the heated wire was the same in all.

Dr. Wreden, of St. Petersburg, proposed, in addition to cutting out a large piece of the membrane, the removal of a portion of the lower end of the malleus, and constructed two very ingenious instruments for effecting his purpose. The one for cutting away the membrane consists of a handle supporting at an angle of about 70° a knife, the blade of which is made to describe a perfect circle by pressure upon a slide moving in the handle. The other, for excision of the bone, is a pair of stout shears for cutting, and below them a light pair of forceps for seizing and extracting the excised portion: both slide in a tube after the manner of the branches of the lever-ring forceps used for the extraction of polypi.

In the comprehensive and valuable work upon aural surgery by Dr. Jos. Gruber, of Vienna, just published, the author describes an instrument of his own, to which he gives the name Myringectome, and which serves not only for cutting away a portion of the membrane, but also for Wreden's operation of Sphyrotomy. It is modelled upon the principle of the lithotrite, the end of the stationary blade being circular and fænestrated; into the opening the end of the movable blade fits like a punch. In operating, a slit is first cut through the membrana tympani with a knife or paracentesis needle, the lower blade of the myringectome introduced through the opening and brought in contact with the inner surface of the membrane; the upper blade, being forced downward by means of a small screw, punches out a circular piece which may be extracted between the blades of the instrument. This procedure may be repeated until an opening of the desired size is obtained.

A short time before his experiments with galvano-caustic, Dr. Politzer had succeeded in establishing a permanent opening in the case of a woman who had suffered from acute purulent inflammation of the middle ear, accompanied by perforation of the membrane. The inflammation subsided, the discharge ceased, and the opening into the tympanum remaining, she was able to hear distinctly. Subsequently, however, the opening became closed by cicatricial tissue and the hearing greatly diminished. While in this condition she applied to Dr. Politzer, who made an incision through the cicatrix, dilated it by means of a sea-tangle tent, and then inserted a small hard rubber tube, having a groove cut in it for the reception of the edges of the wound.

Forty-eight hours after the operation, the tube or eyelet was firmly in place, the edges of the opening having closed around it. Before the operation a test watch was heard only when brought in contact with the ear; the same watch was now heard at a distance of nine inches. This improvement continued gradually to increase during the next six months, at the end of which time the hearing was very nearly normal.* The circumstances of Dr. Politzer's case greatly favored success. The membrana tympani was at no point adherent to the opposite wall of the tympanum, nor was it particularly concave; the eyelet was inserted in a thin elastic cicatrix formed by the outer and inner coats of the membrana tympani, and not in a membrane thickened by an old inflammatory process and rich in bloodvessels, and the meatus admitted a large sized speculum. We cannot expect to find equally favorable conditions in all the cases to which this operation is applicable. The

* An account of the operation, together with the substance of Dr. Politzer's article upon the subject of permanent artificial openings in the membrana tympani, was published in the *Boston Medical and Surgical Journal* of March 25th, 1869, to which I would refer for a fuller explanation of the method of procedure.

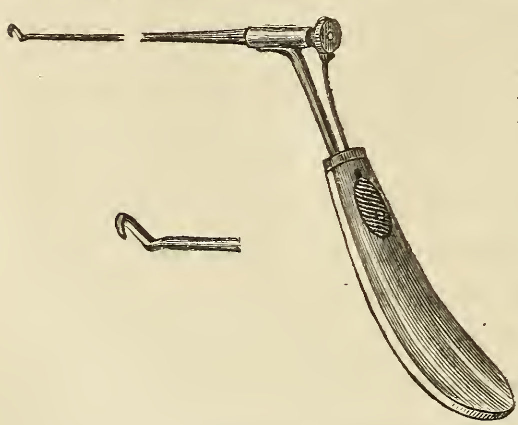
eyelet must necessarily project into the tympanic cavity, and as the distance from the promontory to the membrana tympani is only about 3 m. m., a very slight concavity of the latter would oblige us to choose as the point for insertion a portion of the membrane corresponding to a recess in the opposite wall, either the anterior inferior segment near the opening of the Eustachian tube, or posteriorly below the incus and near the periphery, if we would not run the risk of having the inner end of the eyelet come in contact with the tympanic wall; in which case closure of the opening by secretion would be the least evil which we might expect. Where the malleus is in contact with the promontory, the difficulty is still greater, and it may be necessary to cut the manubrium free from the promontory to which it is adherent, as preliminary to the insertion of the eyelet.

Subsequent observation of Dr. Politzer's case developed a very interesting fact. Five months after the operation it was found that the eyelet which had been inserted near the malleus had moved in the direction of the radiating fibres of the lamina propria, and lay close to the periphery of the membrane, its outer end resting on the posterior wall of the meatus. This observation, in addition to its physiological interest, is of practical importance and adds another complication to Politzer's operation; for, knowing that if inserted near the centre of the membrane it will certainly in course of time reach the periphery by the shortest course, the eyelet must be so placed that in its wandering the inner end may not come in contact with the descending process of the incus or any of the projecting parts of the tympanic wall. Notwithstanding the difficulties besetting this operation, and the fact that though performed with all judgment and delicacy a favorable result cannot be insured on account of the liability of subsequent inflammation and stoppage of the opening in the eyelet, we should not hesitate to resort to it when there

is a prospect of success, because there are so many cases where the establishment of a permanent opening is the only means of which we have at present any knowledge whereby the hearing may be restored, or the patient freed from subjective symptoms, which are sometimes so severe as to entirely preclude mental labor and even induce insanity.

The operations confined to the cavity of the tympanum are very few in number. Of these, the removal of polypi and of adhesions, as already mentioned, and tenotomy, are the most important. In consequence of long continued closure of the Eustachian tube, the air within the tympanum is gradually absorbed, and the pressure of the atmosphere externally forces the membrana tympani inwards and maintains it in that position ; the tensor tympani muscle is relaxed and finally becomes retracted, so that even after the permeability of the Eustachian tube has been re-established, the membrane, held by the muscle, retains its abnormal position ; it is in this condition that the operation of tenotomy is applicable.

Weber's tenotome* closely resembles the knife which Wreden employed for the first part of his operation, except that for the knife blade, a small hook with a cutting edge is substituted. An opening is made through the membrane in front of the malleus, and the hook introduced and caught



over the tendon of the muscle close to its insertion. By operating the slide in the handle of the instrument, the hook is made to describe a quarter of a circle, and cut through the tendon. From operations performed with Weber's instrument upon the cad-

* Monatschrift für Ohrenheilkunde, December, 1868.

aver, I should question the advisability of employing it upon the living subject. Unless handled with the greatest care there is apt to be extensive laceration of the membrane, and the shape of the hook is such that considerable force is sometimes required before the tendon can be divided.

Politzer has proposed hermetically closing the meatus in cases of retraction of the tensor tympani, when there is passage for air through the Eustachian tube. The meatus is tightly plugged with a wad of cotton wool soaked in oil or smeared with simple cerate, which is allowed to remain twenty-four hours at a time; the air between the plug and the membrana tympani is rapidly absorbed, and the air within the tympanum exerts a steady pressure outwards. In many cases this simple procedure is very effectual in relieving the tension of the muscle and restoring the membrane to its proper position.

Since the first use of the catheter, introduced through the nostril,* the method of treatment of diseases of the middle ear through the Eustachian tube, and the treatment of affections of the Eustachian tube itself, have remained very much the same.

Since Cleland's time the catheter has passed through divers modifications, which it would be superfluous to notice here. The instrument, in its present form, serves not only for diagnostic but also for therapeutic purposes. By its use in connection with the otoscope, we are enabled to determine the condition of the Eustachian tube and middle ear, and diagnosticate the existence of small openings in the membrana tympani. Through it we may introduce medicated solutions and vapors, and where the condition of the passage demands it, bougies for purposes of dilatation—the small catgut, laminaria and the finer sizes of the French olivary bougie

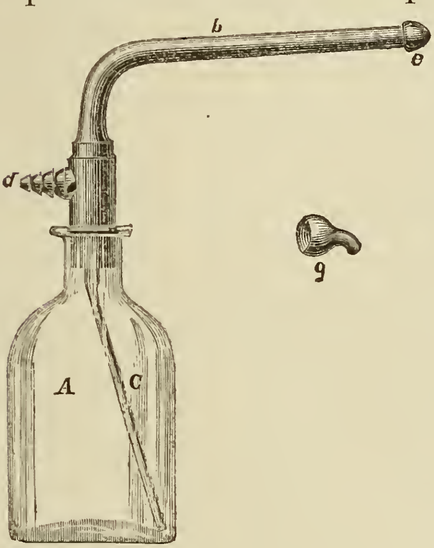
* By Cleland, 1741.

being most commonly employed. Of these, the latter is preferable, as it is passed more readily than the cat-gut, while the degree to which the laminaria will dilate is very variable, and there are cases on record where laminaria bougies have broken in the Eustachian tube, an accident the possible results of which we cannot contemplate without dread. In injecting medicated solutions through the catheter, it is a question as to how much of the fluid reaches the middle ear, though the quantity injected through the catheter at different times may be the same: a varying proportion runs down into the throat, according to the position of the beak of the instrument and the degree of permeability of the Eustachian tube.

With medicated vapors the same question holds good, and it is with the view of furnishing a reliable means of medication per tubam that Weber has devised the double catheter described in the fifth number of the Monatschrift fur Ohrenheilkunde, 1868, and to which he gives the name "pharmacokoniantron." A silver catheter being introduced in the usual manner, a small flexible catheter, having a lateral opening at the beak, is passed through it, up the Eustachian tube and into the tympanum. At the outer end of the flexible catheter is a mark corresponding to the lateral opening in the beak. With this for a guide the opening can be turned in any desired direction, and the fluid introduced into the catheter be ejected in the form of a fine spray by means of air forced in with a balloon. As a substitute for catheterization, which is sometimes impossible, and always disagreeable to the patient, Dr. Politzer introduced the use of a rubber balloon, having a short curved beak attached to it by a flexible tube; this being passed into the nostril and the alæ closed upon it by the thumb and forefinger of the left hand, the patient is made to swallow; at the moment of contraction of the muscles of the pharynx the balloon is compressed. The air which is forced

in at the nostrils cannot make its exit by the same passage, and cannot escape down the throat; it must, therefore, pass up the Eustachian tube and into the tympanum. This constitutes one of our most valued means of treatment at the present time.

In addition to local treatment in affections of the middle ear, a due attention to the condition of the throat and nasal passages is often of equal importance. For the purpose of making applications to the naso-pharyngeal space, and as a substitute for the nasal douche, which sometimes cannot easily be borne, and does more harm than good in such cases, Leiter, of Vienna, has constructed the atomizer, represented in the accompanying woodcut. The instru-



ment is of hard rubber and consists of a tube *b*, enclosing a smaller tube which communicates with the dip-pipe *c*. At *d*, a flexible tube with double bulbs is connected, by which air is forced into the atomizer. At *e*, mouthpieces of different shapes can be attached by screwing them on to the tube, and by varying the position of the openings in the mouthpieces the jet of spray can be turned in any direction. By removing the mouthpiece a stream of water can be substituted for the spray. The whole apparatus fits like a stopper into the bottle *a*, containing the medicated solution. With the mouth-piece represented at *g*, applications may be made not only to the upper part of the pharynx, through the mouth, but also to the larynx and vocal cords.

The consideration of diseases of the labyrinth, and, in this connection, of the experiments of Brenner and others in the application of the electric current to diagnosis and treatment of labyrinth affections, though exceedingly interesting, would of necessity be lengthy, and moreover would scarcely come within the scope of this communication. The same may be said of the different forms of parasitic growth in the external meatus, and either subject alone would afford material for a separate paper.

PUBLICATIONS

OF THE

MASSACHUSETTS MEDICAL SOCIETY.

VOL. III.—No. III.

BOSTON:

PRINTED BY DAVID CLAPP & SON.....334 WASHINGTON ST.

MEDICAL AND SURGICAL JOURNAL OFFICE.

1871.

NOTICE.

The delay in issuing the present number of the Publications has been due to the time necessarily taken by "experts"—the transmission of proofs, and their correction by the several authors—printer's engagements—and the non-receipt till within a day or two of the plates for the last Paper in the collection.

It is the intention of the Committee on Publications to publish, hereafter, in one volume annually, all the documents usually printed by the Society under the titles of "Communications," "Publications," and "Proceedings."

ESSAY

ON

" *An effective and ready method of ventilating sick rooms—one that can be put in operation at once, at the moment needed, with least difficulty and expense, in houses of ordinary construction,"*

FOR WHICH A PRIZE WAS AWARDED

BY THE

Massachusetts Medical Society,

TO

x. y. z.

1871.

ESSAY ON VENTILATION.

Whatever opinions may be held among enlightened physicians as to the nature of disease, its causes and mode of treatment, all agree as to the very great importance to the sick of fresh air and cleanliness; in other words, the removal of impurities, volatile and solid. Indeed, by proper sanative measures such great relief has been obtained from those sufferings, sometimes more formidable than the disease itself—sufferings not the necessary accompaniments or consequences of disease, but solely the effects of bad management as regards air, heat, light and cleanliness—that some have been lead greatly to undervalue the usefulness of medicines. But competent persons who carefully consider the extent to which medicines prevent or shorten some diseases, relieve pain and other equally great and unnecessary evils, support the strength and modify the results in most others, must admit, without assigning to each its exact value, that sanative measures and the judicious use of medicines combined produce effects not reasonably to be expected from either singly.

Ventilation is important to the sick and aids materially in recovery. It is also important to the nurse, and the more constant her duties the more necessary does it become. Her duties are exhausting; impure air, even in ordinary sickness, also exhausts; in infectious diseases, the state of things is still worse. It is well known that infection requires a cer-

tain intensity and a certain length of exposure to produce its effects. It is also well known that sufficient dilution effectually destroys infection, otherwise no measures ordinarily taken would prevent its indefinite spread. Dilution with atmospheric air certainly prevents the spread of all diseases dependent upon *volatile* emanations from the sick.

Those visiting the sick will, if their rooms are well ventilated, be less likely, for the reasons just mentioned, to carry away an infectious atmosphere, absorbed by their clothing, especially if the outer garments are buttoned closely and the head covered while in the room, and these coverings thrown open and exposed to the outer air as soon as it is reached. The hands also should be washed. Let the opposite course be followed, and infection may be carried about for a considerable time and to considerable distance.

To the sick, to the nurse, and all others who come in contact with the sick, ventilation is important.

The management of the sick room in these matters now in hand involves a succession of varying details. It is impossible to anticipate and lay down rules for all these details. The best that can be done is to make certain suggestions, leaving their successful application to the watchfulness, accurate observation and good judgment of the nurse.

The efficiency of any plan for ventilating a room depends much upon its situation. Select, if possible, a corner room, with windows upon two sides—a south-east or south-west exposure is most desirable. It should command direct sunlight and a pleasant aspect, both often valuable helps in recovering from disease. Avoid a room that faces a high dead wall; it cuts off air, light and view, and tends to produce a constant dampness of walls and ground. The windows should have outside blinds.

Large square rooms are the best; it is more difficult to ventilate a small room without draughts than a large one;

bed rooms are poor, and a bed room without a window is to be avoided as the worst—it is only a closet. The second floor is generally more desirable than the first; it is more quiet, drier, has freer air, better light, and if the room below is occupied, has a warmer floor in winter. An open fire-place with a good chimney, properly managed, is the most efficient moving power for ventilation; it is hardly possible to succeed well without it; therefore secure a room with a fire-place, or at least an opening into a chimney. Remove the fire-board and see that the flue is open to the external air; it is often closed at the throat to prevent the falling of soot, or by accident, and sometimes it is closed at the top; it is well to try the draught with a lighted candle. If there is an opening into a ventilating flue near the ceiling, sufficient air must be provided for this and the fire-place, otherwise smoke and suffocating gases may be drawn down through this very flue into the room.

All offensive matter should be removed from the immediate vicinity of the sick room, whether this offensive matter is on the ground or in the receptacles of filth—swill pails or dust barrels. Especially should these things be attended to on the side of the prevailing winds, lest the air admitted for ventilation be worse than that of the room to be ventilated. The entries and passages must be kept clean, well aired, and the entrance of impure air from kitchen, privy, or other source of impurity, cut off.

The floor must be kept clean. Carpets are objectionable because they retain dust, moistnre and other impurities. In summer a painted floor, or even an unpainted floor, is much better; a rug or piece of carpet at the bedside is all that is necessary, and this is easily removed if soiled or for the purpose of airing. Undoubtedly the carpet keeps the floor warmer in winter, and to most has an air of comfort which in some measure makes amends for its objectionable qualities.

All offensive matter should be removed from the room at once; the chamber vessels, always fitted with proper covers, taken from the room as soon as used, and well rinsed in a place whence the odors cannot again reach the sick room. Cold water in the chamber vessel prevents much of the odor while in use. The best arrangement is a tin tube two inches in diameter leading from the side of the close stool along the floor to a chimney with a good draught. This is effective at all times; no odor can escape into the room even while in use. No odor will escape from a privy vault connected with a warm chimney by a pipe of any material, five or six inches in diameter, provided the vault and connections are tight. It has succeeded where the vault and chimney were fifty feet apart; so also with drains.

Have no more furniture in a sick room than is actually in use. Remove all clothing from the walls, drawers and closets; it is in the way; it absorbs and stores up impure air, and is a hindrance to good ventilation.

All nourishment should be kept in an adjoining room, not only to protect it from contaminating odors which are readily absorbed, but also that it may be prepared and changed without disturbing the sick.

A single bed is to be preferred to a double bed. The French or box bedstead is objectionable; it is not so easily aired, and the sides allow very little circulation beneath it. In a wide bed it is exceedingly difficult to reach or support a patient unless close to its side; no one can do much at arms' length. Two beds for a change are better than any movement from side to side of a wide bed. The iron bedstead is undoubtedly the best. No bed should have a valance; it prevents the beneficial movement of air beneath it.

The position of the bed in the room is important. It should stand sufficiently far from the wall to allow a free circulation of air and a free passage for the nurse. This

position, also, secures the bed from draughts of cold air from windows, much better than covering them with blankets, as is often done.

The bed clothing should be light, that the patient may turn easily beneath it; and porous, that the emanations may readily escape. Cotton sheets and woolen blankets are therefore the best. A good hair mattress is much better than a feather bed. The feathers do not keep in place, and unless frequently beaten up there is soon little beneath the patient but the two sides of the bed-tick. A few folds of a blanket laid smoothly over a mattress makes an even surface and is a great relief to a sensitive and emaciated body. An India-rubber sheet is a good protection, but it should never be in contact with the skin; it is cold and often clammy with condensed perspiration.

The bed clothing should be aired daily. It is in vain to expect all the advantages of ventilation if the body is constantly covered with clothing saturated with perspiration and animal effluvia. The lungs are not the only breathing organs; the skin to a less extent is also a breathing organ, and it must be kept clean and supplied with pure air. Ordinarily a pint of water is exhaled from the skin daily; in some instances with free perspiration it is more than double that quantity; more or less of this is absorbed by the mattress and clothing, and is liable to be reabsorbed by the skin. The clothing, if not changed daily, should be aired daily, hung in the open air, in the sunshine if possible. The light of the sun not only dries, but it is also powerful in decomposing all animal matter. If it is important to air and dry clean clothing before it is used, it is certainly important to air and dry clothing that has just been used. Have a change of mattresses, that these also may be aired, dried and sunned. But a small part of the work is done if this is neglected. They absorb and retain more animal

effluvia than the bed clothes. By this airing we not only discharge what ought to be discharged, but also bring in a stock of pure invigorating air. This care of the bedding is highly important in infectious diseases and long-continued diseases, like typhoid fever. It is perhaps hardly necessary to add that all airing should be done outside the patient's room.

It would be useful and not difficult in some offensive diseases to keep up a slight but constant ventilation of the bed itself. A tin tube two inches in diameter, leading from the foot board along the floor to a chimney, will accomplish this with very little trouble. The patient should meanwhile be kept warmed by artificial heat in the bed if necessary, for ventilation is a cooling process.

By ventilation is meant *a plenty of pure air and no chill.* Chill is the great dread of the nurse, and the one cause of chill, in the opinion of most nurses, is fresh air. Soldiers are not chilled or injured in hospital tents; on the other hand they often get rapidly well when removed from a barrack hospital to a tent. In some of our best appointed civil hospitals patients have made little or no progress till placed in the open air under canvass. A person with delicate lungs may leave a warm house, go at once into a tent, live there night and day, and so far from suffering a chill, rapidly improve; and yet the external air is constantly moving through the thin walls night and day with very considerable changes of temperature. But if a number of persons leave an over-heated room after an evening party and go into the cold air, the effects are widely different; one may have a sore throat, another a cold in the head, others a pneumonia or an inflammation of some internal organ. The cause in all is the same; a sudden blast of cold air upon the sensitive surface of the heated body. So also a chill will be produced by a blast of cold air upon one part of the body while other parts are protected or over-heated.

What we are to avoid, then, are sudden changes of temperature and partial draughts. A constant, general, moderate movement of air of a proper temperature is safe day or night.

When a patient is warmly covered in bed, free ventilation is perfectly safe. But when he is up, especially when weak and after long and profuse perspiration, the skin is extremely sensitive. The windows should then be closed, the temperature raised and the air kept free from offensive draughts. So also when a patient is to be bathed in bed, a part only should be washed at a time, the rest of the body meanwhile remaining covered and the air warm and still.

How can air be freely admitted to a sick room and draughts and consequent chills avoided? In a room not expressly fitted for sickness, mainly by windows. Occasionally by the doors, but never unless the passage ways have open windows, are clean and free from impure air.

Summer Ventilation.—Air may be freely admitted by raising the lower sash and lowering the upper sash. If the upper sash is not movable, as it ought always to be in every house, it is easily made movable by taking out the stops beneath it. Five or six inches of the upper end of the stops can then be cut off and the lower part replaced; the portion cut off being also replaced when the upper sash is entirely closed. This arrangement in a corner room gives a considerable and sometimes a sufficient change of air.

But unless these openings are properly guarded and the incoming air properly directed, a cold or strong wind against the windows will produce unpleasant or even dangerous draughts.

These draughts may be prevented or greatly diminished by the following device.

Fit into the window frame, near the bottom of the window, a thin piece of board six or eight inches wide and long

enough to reach from side to side of the frame. Two pieces of clap-board fastened together by their thin edges make a board which is light, easily obtained, and although not so neat, answers well. Push this piece of wood close up to the sash, but not so close as to prevent its proper up and down movement. Secure it in its place by a wedge, or in any other more substantial manner, so that it may be turned up and down as on a hinge next the sash. Generally the friction at the ends, if the board is light, will hold it in place; if it does not, a string fastened by one of its ends to the upper part of the lower sash and the other to the edge of the board will fix it in any desired position. If the opening is directed downward and the sash raised (never above the upper edge of the board), the entering air will go towards the floor. It is a fact easily demonstrated, that air striking a surface does not rebound from it like an elastic body, but moves parallel to the surface and diffuses itself along it. Air entering in the manner here described soon reaches the floor and gradually displaces the air above it until it fills the room, and this without perceptible draught to a sick person in bed. If the same board be placed with its lower edge upon the window-sill inside the sash and against the frame, leaving a space between the board and the sash, the entering current will have a movement directly upward; it will also enter between the sashes with the same direction. This is the proper arrangement when the patient is sitting up.

To the upper sash, a similar board should in the same way be fitted, also movable, with the opening turned obliquely upward. Air entering at the top of the window then strikes the ceiling along which it moves, is diffused, and all draughts avoided. This arrangement does not prevent the escape of air at the same place, if the current is in the opposite direction. "Millinet" or other open material may

be secured to either board or to the sash, to break the force of the current or to prevent the entrance of rain, insects or dust. A piece of cloth fastened to the lower edge of the lower board will lead the entering air along the wall as directly to the floor as may be desired. Triangular pieces of paste-board, nailed to the ends of the ventilating boards, will give the air a more definite direction. If the windows have no outside blinds, a board, like that described, outside of the sash, directed downward, effectually excludes rain.

Fig. 1.

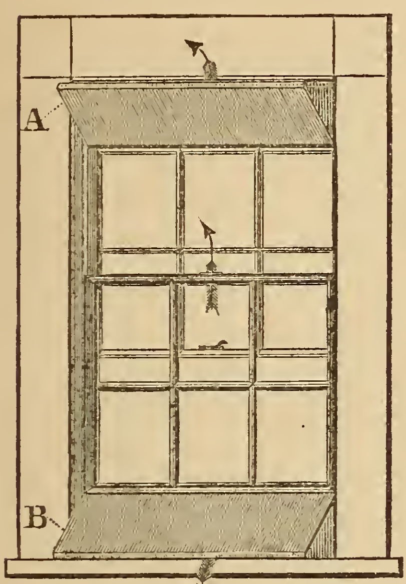


Fig. 2.

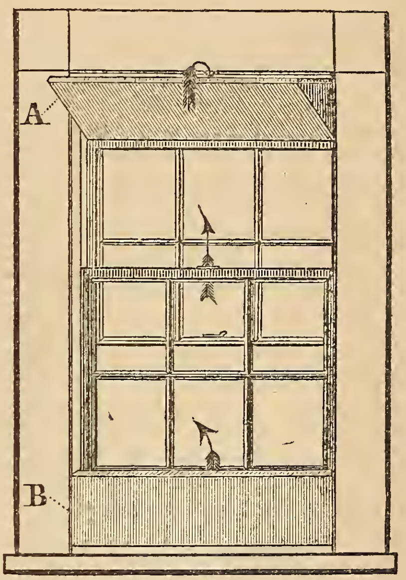


Fig. 1.—Window as seen from within. A and B, ventilating boards in position. The lower board directs the air to the floor, the upper board turns the air upward and diffuses it.

Fig. 2.—Ventilating boards, so arranged that the air is turned directly upward.

By this method air can be made to sweep along the ceiling and along the floor, or in any other direction, without affecting unpleasantly a patient in bed or sitting up. The strength of the current is easily controlled by raising or lowering the sash.

All can in this way enjoy the luxury of fresh air during sleep without danger from sudden changes of temperature or sudden blasts of wind.

When the wind, instead of blowing against the window, is moving parallel to the wall of the house, it is often difficult to secure a current of air into the room. This difficulty can frequently be overcome in the following manner. Take a piece of board sixteen or eighteen inches long and eight or nine inches wide, place it on the sill edgewise against the window-frame and let it project beyond the window, and so fasten it that it shall be turned obliquely towards the wind. Then shut the sash down upon the edge of the board, and the wind will be directed into the room. A similar board placed at the top of the window and turned from the wind, will aid in discharging the air from that part of the room. The efficacy of these boards may be increased by nailing strips of board on each of the two horizontal edges. They form, when thus arranged, a kind of wind-sails, and can obviously be combined in the same window with the boards first described.

Fig. 3.

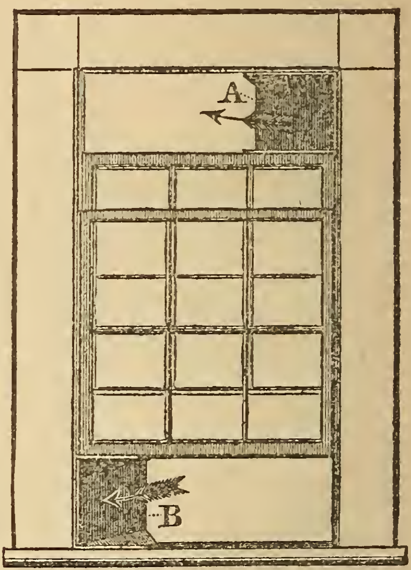


Fig. 3.—Deflecting boards to direct fresh air into the room at the bottom, and vitiated air out of the room at the top.

When doors must be used for the admission of air, a properly arranged screen, a frame, a "clothes-horse," or even a blanket or sheet hung from the ceiling or upon an arm projecting from the wall, will protect the sick and give the current a harmless direction. Great care should be taken lest a door be left open, and a person sitting up be exposed to a draught between it and an open window.

The Outlet for Air.— No air can enter a room unless an equal quantity goes out. Although this is perfectly obvious,

it is very often entirely overlooked in practice. The best outlet is an open fire-place or an opening into a chimney warmed by a fire. No outlet we shall be likely to get in a sick room approaches it in efficiency. A few bricks in a fire-place can be so arranged as to bring the fuel nearer the throat of the chimney, and convert it into a powerful ventilator, without increasing the heat of the room in summer. A grate properly screened need not be objectionable on the score of heat.

The great advantage of this arrangement is that it is constant in its working, whatever the direction of the wind — a result not always obtained by the windows. A gas light constantly burning in a chimney, or what is better, a gas *heater*, or a couple of kerosene lamps, will so warm the column of air as to create a perceptible draught, which, when other means are wanting, should not be neglected.

A chimney warmed by a fire has ordinarily a draught of about three feet a second; with this velocity, through a hole of eight inches diameter, will pass in fifteen minutes the air of a room ten feet square and nine feet high. The throat of an ordinary chimney would, on the same supposition, allow twice this quantity to pass in the same time.

The constant draught into a sick-room to supply the chimney, effectually prevents the escape of air from it into other parts of the house— in some diseases a matter of importance.

If the room has no chimney or opening into a chimney, a certain amount of ventilation can be got by opening the top and bottom of the window, and this can be increased by putting one of the inclined strips of board outside the upper sash, with its opening upwards. It protects the upper opening from a current of air inward, and allows the full effect of any difference of temperature between the external and internal air. Although no care can make this plan equal the open fire-place, it will be found a great improvement

upon the state of things where ventilation is entirely neglected.

Winter Ventilation.—Unless the air is warmed before it enters the room, greater care must be taken to prevent draughts in winter than in summer. An open fire-place at this season of the year is the most desirable ventilator, and generally a sufficient one. Next to this an open stove; the worst is an "air-tight stove," which should never be used in a sick room unless a proper opening be made into the chimney, at least eight inches square, or as large as is practicable without injury to the draught of the stove. An open stove or "Franklin fire-place," is the best means we can ordinarily get for both warming and ventilating a sick room not specially arranged for the purpose. It warms the room in our climate sufficiently, which an open grate frequently fails to do, and as a ventilator is also sufficient. Besides this advantage it can be made to heat the incoming air by a simple arrangement, which will at the same time distribute the heat more equably. Let the stove be surrounded on three of its sides with a casing of sheet iron or zinc; make this casing as high as the stove; place it three inches from the stove and raise it four inches from the floor. To conduct air to it lay down upon the floor and up the wall to the window, two parallel strips of board three or four inches wide, and nail upon them a strip of cloth five or six inches wide. This makes a temporary air-channel of sufficient capacity, which is controlled by the movements of the sash. A tin pipe, three or four inches in diameter, similarly arranged, would be better and more permanent.

Air can also be admitted in the same manner as for summer ventilation. The current must be so directed by means of the boards that draughts are avoided, and the quantity regulated by the movements of the sashes. When the patient is sitting up, the air must be turned directly upward

or it will sweep the floor with its cold current ; when he is in bed, the opposite direction is better.

This method of ventilating sick rooms is simple. The same principle has been applied with success to the ventilation of railway carriages, and to some public buildings. The mechanical arrangements will, on trial, be found capable of a greater variety of applications than at first would be supposed. They give to the incoming air any desired direction ; they regulate its quantity and diffuse it in the least objectionable manner. The outlets for air are also simple, and as a general rule easily obtained and sufficient. They have all the advantages of a complicated system of tubes and valves, while they have the additional advantages of being cheap, easily obtained, easily fitted, and in no degree interfering with the movements of the window sashes. More than this we cannot do, within the limits of the terms proposed. For by these terms, all cumbersome and expensive machinery, with fans or blowers driven by springs, weights or other means, are excluded. Neither can we cut up walls, floors and ceilings, and introduce pipes and flues. The method, by the limitations set forth, must be, like this now proposed, simple, effective, cheap and easily applied.

The question often comes up, Is it safe to admit the *night air?* In certain malarious districts it may not be, but in all other cases sufficient air to keep the room fresh is always safe. Fresh night air will always be safer than night air contaminated with animal effluvia. Patients are no more liable to take cold by night than by day. Doubtless the temperature tends to fall in the absence of the sun and by the admission of colder air, but this can always be counteracted by increased fuel and, if need be, diminished ventilation.

We have no ready test of the purity of air, to warn us of the necessity of ventilation, nor any instrument which mea-

sures it, as the thermometer measures heat, or the hygrometer or "Weather House" measures moisture. The best and most easily applied test we have, or probably shall have, is the sense of smell. But unfortunately this test must be frequently compared with the pure external air, as a standard, or it is useless. Whenever, on entering a room from the outer air, we smell the animal effluvia, or "fever-smell" as it is sometimes called, the air is impure, the room needs ventilation. Instead of using this test, nurses are very apt to appeal to the thermometer, and, if that indicates a low temperature, to declare the air all right. Purity of air and temperature bear no relation to each other, — a warm air may be a fresh air, and a cold air a very impure one.

The proper temperature of a sick-room may be assumed to be 68° to 70° Fahr. Generally, the temperature agreeable to attendants out of bed, is quite warm enough for a patient in bed. Throughout some diseases the heat-making power is much diminished ; in most febrile diseases, during some part of the twenty-four hours it is diminished. A "fever turn" consists of a chill, then a hot turn followed by sweating. After this there is frequently a state of exhaustion, both of strength and heat, requiring care. During a tedious convalescence from typhoid fever, and in many other diseases, after the fever is gone, the temperature falls towards morning, and the weak require food and warmth. Cases, especially among the aged, are unquestionably lost for want of attention in this respect. Piling on clothing does little or no good ; raising the temperature of the room does not meet the case ; what is wanted is the immediate, direct application of heat by warm stimulating drinks, warm bottles to the feet, or, better still, warm blankets that envelope both feet and legs, and warm cloths to the "pit of the stomach" frequently renewed. The object is not only to prevent the loss of heat, but also, what

is more important, supply from without that necessary heat which the body is too weak to make.

A word of caution. Many patients unconscious or nearly insensible from typhoid fever or affections of the brain, or under the influence of narcotics, have suffered severe burns of the feet from the careless use of overheated bricks or bottles. Nurses are not aware how easily weakened parts are burned. No greater heat than can be borne by the hand, about 120° Fahr., should ever be applied, if we would have our patients safe from burns.

Moisture in winter may, often with advantage, be added to the air of a sick-room. A vessel of water upon the stove will serve a good purpose, but it must be clean and fresh and in sufficient quantity. In the treatment of disease attempts are sometimes made to moisten the air to saturation : these attempts generally fail unless the windows and walls are covered with blankets or other non-conducting substances and the quantity of steam is very large. If the room is properly ventilated, without which the foul air is apt to do more harm than the moisture does good, the requisite amount of steam can only be obtained from a boiler of considerable power.

Light as well as fresh air is needed in a sick-room. All know that plants will not thrive in a dark room. The sick, especially during convalescence, require light as much as plants ; not only light, but direct sun-light. Its warmth is pleasant, its associations are pleasant ; but it has other influences we cannot explain. It aids ventilation, it warms and dries the room and renders healthful what otherwise is poisonous. The pale, weak and bloodless, under the direct influence of a "sun-bath" gain color, strength and health. Not that all are to be exposed to it under all circumstances, but let the room have a sunny aspect.

ALOPECIA.

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OF BOSTON.

READ JUNE 6, 1871.

ALOPECIA.

Alopecia is derived from the Greek word for fox, these animals being subject to a loss of hair from the head. Let us hope that their proverbial reputation for wisdom has some better foundation. Since the time of Absalom, however, the world has lost in hair what it has gained in wisdom, and to-day baldness has become an evil well meriting the attention of the medical profession.

Baldness is the absence of hair from parts normally supplied with it, due to atrophy of the hair, the result of a "morbid interference with its typical growth,"* arising either from a derangement of its nutrition in general, causing deficient hair growth, or from a structural alteration of the individual hairs. Deficient hair growth may be congenital, alopecia adnata; or acquired, alopecia acquisita.

Congenital alopecia may be partial or total, the skin being smooth and unchanged. The teeth are generally developed very late. Danz† mentions the cases of two adult Jews, in whom the teeth, as well as the hair, were absent. As a rule, we observe coincidence of abnormality in the dermal covering and in the teeth. Witness the edentata and cetacea. According to Darwin,‡ a deficiency of teeth was found in three hairless "Egyptian" dogs and in a hairless terrier.

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* Kohn (M.). Wiener Med. Presse, No. 44, 1870.

† Stark's Archiv f. d. Geburtshülfe, p. 884, Bd. 4.

‡ Darwin. Animals and Plants under Domestication, vol. ii. p. 326.

So the rare cases where hair has been renewed in old age have usually been accompanied by a renewal of the teeth. Julia Pastrana, the Spanish dancer, had a full beard and her whole body was hairy. She had also in both upper and lower jaws an irregular double set of teeth, one row placed within the other. A family at Ava, in which through three successive generations a universal hair-growth was present, has been observed at three different times by different trustworthy authorities. Crawford,* in 1828, saw the first of these generations, a man covered everywhere with fine hair like an ape. He came to puberty at 20 years of age, at which time he shed his infantile teeth. The set which followed never consisted of more than the incisors and left canine in the lower jaw; and in the upper, four teeth, the two outer ones partaking of the canine form. His youngest girl, aged 2½, was covered with fine silky hair, and had only two incisors in each jaw. Capt. Youle† saw the family in 1855. The girl, now a married woman, was covered with hair, even upon her nose, like a Skye terrier, and had never had either molar or canine teeth. Her child, aged fourteen months, was evidently taking after his mother, and tallied with Crawford's description of the woman herself as a child. In 1867, the family was seen and photographed by Capt. Houghton.‡ Usually, in cases of congenital deficiency, the hair makes its appearance during the first year partially or completely. One such case, however, was noticed in 1827, at La Charité hospital, by Rayer,§ in which the whole body remained bald, except for a few lanugo hairs. The patient, when seen, was 32 years of age. His father had suffered

* Crawford, John. "Journal of an Embassy from the Governor-General of India to the Court of Ava. 1868."

† Youle, Capt. Henry. "Narrative of the Mission sent by the Governor-General of India to the Court of Ava."

‡ Beigel. Human Hair. 1869, p. 67.

§ Rayer. 2d edit. (Eng.), p. 1049.

in the same way, and as other similar cases are reported it is possible that some hereditary cause may exist.

Alopecia acquisita is characterized by excessive falling out of the hairs and by defective reproduction. The relative value placed by different authors upon these different characteristics has given rise to great confusion in nomenclature. I refrain from an enumeration of the unpractical Greek and Latin titles in use from the time of Celsus to the present day, and adopt once more Kohn's division into Alopecia senilis and Alopecia prœmatura, as the most descriptive and comprehensive.

Alopecia senilis is more common in men than in women, affects by preference the top of the head, and begins, as a rule, just over the occiput or at the forehead. For a time, we find the openings of the hair follicles patent and more or less lanugo hairs. Alopecia senilis is usually preceded by grayness of the hairs, though the presence of the latter by no means presupposes the subsequent occurrence of the former. It is due to the general decay of the vital forces, and any anatomical alterations of tissue-structure are rather to be regarded as sequences than as causes of the baldness. These alterations are, shrivelling of the elements of the hair follicles, and fatty degeneration of the cells of the hair root-sheaths, more marked in proportion to the duration of the baldness; a condition of things self-evidently wholly inimical to any subsequent reproduction of hairs. This form of baldness is therefore incurable.

There remains for consideration our second division, premature baldness, the one most interesting and of most practical importance to us as physicians, being the one we are continually called upon to treat, and, fortunately, the one also most amenable to treatment. Kohn distinguishes two classes, basing his division upon the causes of the mal-

ady : I. Alopecia præmatura idiopathica, due to disordered nervous influence ; and II. Alopecia præmatura symptomatica, the symptom or sequence of a recognizable, organic, morbid condition of the skin, especially of the hair follicles, or also of the hair itself. These two forms vary in their development, course, duration, outward appearance and amenability to treatment.

Idiopathic premature baldness is that form in which the hairs fall out, their reproduction being also inadequately proportionate to their loss, as a consequence solely of nervous disturbance and without demonstrable previous disease of themselves or their appurtenances. This form has been known under various names for more than a hundred years, and it is somewhat remarkable that its present name, Alopecia areata, is also the name under which it was first mentioned, in 1768, by Sauvages. The first real description and delineation of the disease we owe to Willan and Bateman, who, in 1817, described it under the title Porrigo decalvans. For years subsequently, nevertheless, it was confounded by Alibert, Mahon, Gruby, Cazenave, &c., with Herpes [or Tinea] tonsurans, the Porrigo scutulata of Willan, a parasitic disease ; simply because both maladies presented more or less the forms of circles deprived of hair. In Alopecia areata, however, the circular patches are completely bald, while the hair in the immediate neighborhood seems as thick as elsewhere ; the skin of the bald spot is smooth, shining and white. The spots increase in size, run into one another, and may produce extensive baldness lasting months and even years. In Tinea tonsurans the spots are not bald, but covered with hairs broken off short. The skin shows blisters, pustules or scales, and the duration of the disease is easily influenced by treatment. When the hairs are absent, the bald spot shows plainly the openings of the

hair follicles stretched by spores, whereas in Alopecia areata these are atrophied. With Tinea tonsurans we are apt to find also upon the face and neck, and even body, similar circular patches, varying in extent, and due, by microscopical examination, to the same parasite. Hebra, in his "Atlas of Plates, 1858," lays great stress on the distinction between these two diseases.

At present, the clinical appearances and treatment of Alopecia areata are well understood, while with regard to the etiology, two most dissimilar opinions prevail: some dermatologists attributing it entirely to disordered nervous influence; others, to the presence of a parasite. The latter may possibly be the exception, the former is certainly the rule. Virchow* says, "The afferent artery, which supplies a whole series of papillæ in common, may by means of its nerve be brought into an altered condition, so that a contraction or dilatation, and in correspondence with these states a diminished or increased supply of blood to a considerable district takes place." The nutrition then being affected, we get changes in the functional formative processes of the papillæ of the hairs. Let us examine the comparative merits of these two views.

The list of those believing in the parasite as a cause of the disease is a short one: Gruby, Bazin and Hardy in France; and in Great Britain, Tilbury Fox, Hutchinson, Hillier and Anderson, all of wavering faith. Dr. White, of Boston, also holds that in a few rare cases the cause is parasitical. All others disbelieve in a parasite as a cause. I hold with the latter class until I see more positive proof than has yet been adduced, for, though a small amount of positive outweighs a vast amount of negative evidence, personally I have been singularly unsuccessful in my attempts to obtain

* Cell Pathology, p. 246.

from the above-mentioned "defenders of the faith" any of their "positive evidence." For me, therefore, the parasite is, to say the least, "not proven." Let us see what the alleged arguments in its defence amount to. Gruby,* in 1843, announced the discovery of a microscopical fungus, which he dubbed Microsporon audouini. But this is mere assertion,† and, in fact, the only argument at present adduced by the adherents of the fungous theory is the purely negative one of asserting that all cases where no fungus is found are cases in which the disease is at a period of development where the spores are nil, or so small as to elude observation. But positive proof of fungous existence no one has yet given.‡ Gruby's case was probably a Tinea tonsurans, or an Alopecia areata accompanied by seborrhœa, or an accidental case of complication with some fungus wandering at large in the atmosphere. One case is hardly enough to justify us in adopting his views, particularly when we consider the confusion at that time still existing between Alopecia areata and Tinea tonsurans, the latter of course containing fungi. Add to this the inferiority of the microscopes of that day and the lack of suitable means for such a powerful transmission of light as would enable one to examine the interior of the hairs. Moreover [I quote Bazin himself], "the larger portion of the memoir of M. Gruby is only a romance." It is precisely such pseudo-scientific, romantic, visionary theories of Frenchmen, that patient, laborious, exact German investigation has been for the last twenty years successively and successfully strangling. The material fall of Paris is typical of a new era in more ways than one.

* Comptes Rendus, 1843, t. xvii. p. 301. Vide, also, Malmsten; Archives de Mueller, 1848, p. 7.

† Centralblatt f. d. Med. Wissenschaften, 1871, No. 9.

‡ Erasmus Wilson, F.R.S. Diseases of the Skin. Ed. vi. p. 722.

Bazin* himself seems to have lost his reason and literally wallowed in spores. Not content with one sort of fungus, the Microsporon audouini, whose results he must needs subdivide into two forms, he added another, the Microsporon decalvans. This latter parasite, existing only by his bounty, he was compelled to thrust forth and renounce in his edition of 1862, and his two forms resulting from the former, he is also at last content to classify under one head, "Teigne pellade," though even of this he must needs make a true and a false. Being reduced to one fungus he demands all the more the liberal distribution of this one, and asserts its existence in the skin and nails, causing local disease of these parts. This theory he took first on trust, and, when at last he met with a case, was compelled to confess its "similarity to the disease of the nails produced by the Trycophyton of Tinea tonsurans." The coexistent "Pellade" of the head loses rather than gains by this discovery. Let me quote a remarkable sentence of M. Bazin.† "Le Microsporon épidermique et le Microsporon unguéal [si tant est qu'il existe] sont faciles à constater." If the latter is so "facile à constater," why does he have any doubt of its existence ? Finally, the fact that he speaks of the fungus at all as so very readily to be proved by observation, while the best observers almost to a man are utterly unable to find it, looks as if M. Bazin must be mistaken in regard to the identity of what he finds with a fungus, or that he confounds Alopecia areata with the parasitical disease Tinea tonsurans. I have myself seen him make this last mistake and proved him in error by the aid of the microscope. Moreover, to substantiate his theory, he puts but a single witness on the stand, and that one a most wretched-looking

* Basin. "Recherches," 1853.

† Bazin. Affections Cutan. Parasitaires, p. 219.

nondescript sort of a hair upon which are formations which he chooses to say are spores, and that too of a peculiar kind. But he gives no history of the case, no explanation even of how it was obtained, prepared, examined, &c., nothing but his bald assertion that this is the fungus. I will add incidentally that nine months diligent attendance at M. Bazin's cliniques convinced others as well as myself of the non-existence of a fungus in this disease.* But enough! "nil de mortuis!" and when Dermatology and M. Bazin were younger than now they were also better friends.

Hardy follows Bazin, asserts as he does, speaks of the fungus as always to be found, &c., but gives no proof. I cannot find a single report by him of actual examination of any one case. Moreover, in his "Parasitic Diseases,"† he says, "is the general malaise to be attributed to the fungus as an absorbent of the nutritive juices, or is the alteration of nutrition the primary evil and the development of the fungus a mere consequence? As yet, these questions have received no satisfactory solution."

Of Tilbury Fox I speak with deference as a man, able, clear-headed, practical and unprejudiced. Still, my conviction is that he is in error. During an attendance of three months at his clinic, he, with characteristic politeness, gave me every opportunity of studying Alopecia areata, but I could never convince myself of the presence of a fungus. The appearances yielded as a rule to the application of potassa and ether, and in his plate of Alopecia areata,‡ the appearances are rather of fat-globules and detritus than of sporules and sporidia. Mycelium, he confesses he has not

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* Duhring. "Alopecia Areata," Am. Journal of Med. Sciences, July, 1870; and Anon., Lancet, vol. ii., 1870, No. 31.

† Hardy. "Maladies Parasitaires," p 182.

‡ T. Fox. "Skin Diseases of Parasitic Origin." London, 1863.

met with,* and adds, "It appears then that what would have become Tinea tonsurans, and the like, in the young, forms Tinea decalvans [Alopecia areata] in the adult, in consequence of the difference in the character of the soil in the two cases; and whenever in the child the proper nidus is below par, so to speak, that is, similar to that of the adult, Tinea decalvans results." Now I certainly do not deny that fungus will still be found in Tinea tonsurans, by whatever other name this disease may be called. This question of difference in soils is an important one. If we admit it, we reduce all parasites to one; that one, however, remains the Trycophyton of Tinea tonsurans and T. circinata. T. mentagrophytes is now known to be due to the Trycophyton. Ditto the T. sycosis.† It is the Trycophyton which is found in Eczema marginatum.‡ So in the "Chinese Ringworm" of the genitals, the Mentagrium of Martial§ and the Pudendagrum of Pliny, "where the Trycophyton was carried by 'Libertines,' called Cunnilingi and Basiatores, from the face to the genitals, and received again by others from the genitals upon the face."|| It is Hebra's theory that all parasites are identical.¶ Dr. John Lowe** reports a case where favus during cure passed into Tinea tonsurans, from contact with which he himself acquired a Tinea circinata. Hutchinson†† reported a case to the Path. Soc., Dec. 17th, 1861, in which he holds that a Tinea versicolor resulted from the implantation of the Trycophyton. Fox mentions

* Ibid. Loc. cit., pp. 57 and 25.

† Gibert. Traité Pratique des Mal. de la Peau, t. i. pp. 278-279.

‡ Pick. Archiv f. Derm. u. Syph. 1869, 1 Heft. p. 61.

§ Epigram, lib. xi. p. 98.

|| Kuchenmeister.

¶ 2d Plate. New Sydenham Society.

** Lancet, Oct. 29, 1859.

†† Med. Times and Gazette, Jan. 12, 1860.

also many other cases of various effects produced by the same parasite, and *vice versa*. Now if in the one or two least disproved cases of fungus found in Alopecia areata this fungus were a fact, it was probably simply the Trycophyton somewhat modified by the previously existing disease—for I do not deny that the Trycophyton may fall on any part of the body—and cases of its presence here should be, as they most certainly are, the rarest of all, since the field presented for its reception is the smallest, quantitatively, at best, to say nothing of its rarity of existence at all. The disease Alopecia areata remains, however, non-parasitic, whether a subsequent development of fungus takes place or not, and whether that fungus, if developed, be one alone or one of a thousand.

Hutchinson is one of the most able and versatile of our profession. But the very multiplicity of his labors leaves no time for exact research, and he is a specialist neither in the microscope nor in dermatology. However valuable his opinion in general is, the present is a question of fact simply, and only to be decided by the microscope. He gives no proof of a fungus, merely adducing some few cases tending to show that the disease is apparently contagious.* It is a great mistake, an injustice towards others, to accept as truth the mistakes of our superiors, because they are our superiors, and then to disseminate these errors with the additional weight of whatever value our opinions may have. The true majority is not the numerical majority of one leader and an unthinking mass of followers, but lies in what is often quantitatively the minority, viz., the smaller number numerically, but of which number every man is an independent thinker and investigator. For me, therefore, there remain only the cases seen and described; the number of the writers accept-

* Pathol. Soc. Transactions, vol. xiii., p. 266.

ing these views without original investigation is a matter of no importance. And this very subject of Alopecia areata furnishes me with a good example of the authoritative value of mere numbers. Robin* says of this supposed fungus, and, by the way, he never even pretends to have seen more than one case, "Il diffère du Trycophyton tonsurans par des branches nombreuses, courbées, ondulées, &c." Now comes Kuchenmeister† with an exact translation of Robin, without stating, however, that he himself has ever seen a single case. "Die Unterschiede von Trycophyton Tonsurans bestehen in den Zahlreichen, gekrümmten, wellenförmigen Aesten, &c." And, finally, "in the lowest deeps a lower deep," comes Lankester,‡ the translator of the translator, and who never saw a case of the fungus at all, who renders into English the passage I have just quoted, but with the trifling error of exactly transposing the signification. He says: "The distinctive character *of* Trycophyton tonsurans consists in its numerous, curved, undulating branches, &c." It should read, speaking of the Microsporon audouini — its differences *from* the Trycophyton tonsurans consist in its, &c. And thus is Pseudo Science constructed!

Anderson quotes Bazin as a part of the history of the subject, but says he never found any parasite himself, although of the 7342 cases of skin disease at his Glasgow Dispensary, 100 were Alopecia areata. Moreover, throughout his whole article on Alopecia areata,§ he expresses the greatest doubts with regard to the existence of a fungus.

Hillier says:‖ "The sporules of the fungus *are stated to*

* Vegeteaux Parasites, p. 427.
† Die Parasiten, vol. ii. p. 43.
‡ Sydenham Trans., vol. ii. p. 153.
§ Parasitic Affections, p. 138.
‖ Handbook of Skin Diseases, p. 285.

be," "the filaments *are said to be,"* &c., "I found a number of cells *looking like* vegetable spores." He gives a plate of these *au naturel*, and describes them as "having all the appearances presented by the fungus of Tinea tonsurans," being too large for the parasite of Gruby. And all this despite the fact that he is the author to whom all others refer for proof of the contagiousness of the disease, he having had in a parochial school at Hanwell, containing 1100 children, forty-three cases of the disease.* Now as these cases were probably, from his own description, Tinea tonsurans, we are not compelled to fall back upon nervous depression, the result of the traditional diet of "Do-the-boys Hall."

There remains only Dr. White's case, as yet unpublished, but mentioned to me in conversation. I am always open to conviction, but this case occurred, unfortunately for me, six years ago, and I had no opportunity to examine it. At best, it would only show the possible, very rare occurrence of a parasite even were this parasite not an altered Trycophyton. I must therefore still adhere to my opinion that the disease is not due to a fungus.

A few words more, and I leave this subject. Were the disease of parasitic origin, we should expect, instead of a small, the very largest amount of fungus present. The unexpected occurrence of the malady, its rapid and often extensive spread, the actual bursting apart of the hair fibres, the oval distentions, and the completely atrophied root, point to an overwhelming amount of fungus, if any at all. And yet, history furnishes us with only three cases in which fungus was found, and even these are doubtful. And yet, Fox speaks of it as "the least expressed form of Tinea." Now, since there is little or no fungus, we should expect less constitutional derangement, if this disease is due to a

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* Lancet, Oct. 1st, 1864, p. 374.

parasite, than in other parasitical diseases. And yet it is precisely from the ranks of the fungus worshippers that we draw proof to the contrary. Mr. Hutchinson* reports forty-two cases of Alopecia areata. Eleven of these were adults. Of these adults, Nos. 2, 34 and 36 [I retain Mr. H.'s numbers] were "dyspeptic;" No. 41 "not so well as usual, health fair;" Nos. 26, 27 and 37, "good health;" No. 28 "had a bad scald-head, health fair;" No. 23 "had a ringworm in infancy, and the hair did not grow well afterwards;" No. 4, "fair, not feeling well;" Nos. 8 and 40, "dyspeptic;" No. 1, "a little losing flesh;" No. 3 "looks underfed;" No. 5 "had water on the brain, which left a squint, almost well when the disease began;" No. 7, "losing flesh;"† No. 10, "delicate, pale, clear skinned;" No. 13, "delicate, ill-nourished, starved-looking;" No. 15, "strumous;" No. 17, "rather delicate;" No. 18, "delicate, has lost flesh;" No. 22, "pale and delicate;" No. 32, "cachectic." Now if these conditions were the consequence of parasitic disease, we should naturally expect the presence of a vast amount of fungus, and the same is true were the parasite secondary or concomitant, on account of the suitableness of the soil. For "experiment [Hannover and Stilling] has shown that the more diseased the subject the better the artificial introduction of the parasite is accomplished."‡ To this testify Bouley, Trousseau, Delafond, Davy, Robin, Hillier, Thompson, Hunt, Bazin, Balfour§ and others.

The lustreless appearance and disintegrating cuticle of the hairs in the early stages of Alopecia areata are also symptomatic of depraved nervous power, causing imperfect nutri-

* Med. Times and Gazette, Feb. 13th, 1858, p. 165.
† N. B. Two sisters of Case No. 7 had coincident "ringworm."
‡ Fox, T. Parasitic Diseases, 1863, p. 23.
§ Med. Times and Gazette, Feb. 27th, 1858.

tion. The replacing of the long hairs which have fallen out by "fine, short, downy ones," looks like the result of fading nerve power. Parasites which had strangled the strong, full-grown hairs would hardly grant life to these little ones, but would indulge in a general "slaughter of the innocents" as well. Hardy's "arrest of development"* could hardly proceed from "the least expressed form of Tinea," for as Lionel Beale† says: "The diseases of man and the higher animals, known to depend upon the growth and development of vegetable organisms, are local affections confined to a part of the body not involving the blood." The impossibility of inoculation is also unfavorable to the parasitic theory.‡

Alopecia areata begins in one or more spots at the same or nearly the same time on any part of the head, rarely first on the beard. The first thing a patient notices is often a bunch of hair in his comb pulled out all at once from the follicles. Were this due to the presence of spores, what a vast number must needs be present to thus crowd out the hairs, and how quickly they must have grown! In such cases we surely ought to be able to detect the fungus if it existed. The bald spot now tends to grow by peripheral progression, several often run together, and the whole head even may be thus denuded. Before the periphery is attacked, however, the hairs there appear quite normal, or even unusually luxuriant, presenting thus a marked contrast to the bald spot, which is paler even than the rest of the scalp, from imperfect nutrition. In Tinea tonsurans, the hairs are broken off, not drawn out, and the patch is dark from

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* Les Maladies Dartreuses, Paris, 1868, p. 181.

† "Disease Germs," p. 75.

‡ Beiträge zur Kentniss der Haare d. Menschen u. d. Saugethiere; Reissner, Breslau, 1854.

the presence of the stumps. In Alopecia areata, the patch at first seems sometimes slightly raised, the tumefaction resulting probably from the pulling out of so many hairs at once; later, a little sunken and shrunk, as is natural, one of its component parts, the hair-roots, having been abstracted, and their follicles, the openings of which were at first visible, being no longer distended, tending of course to fall together. The disease may, in rare cases, proceed so far as even to divest the entire body of every hair. I have myself seen two such cases, and Wilson mentions three, as well as four cases affecting the head, eyebrows, lids and face. As a rule, in the course of from two months to two years the hair is reproduced, lanugo hairs at first appearing and becoming by degrees stronger and darker. The same place, however, may be several times affected before a complete cure. In rare cases, the reparation stops at the stage of the lanugo hairs. The depravation of nerve force often manifests itself also in depression and loss of spirits.

Now as Alopecia areata is not due to a fungus,* what is it? Rindfleisch† gives two plates of hairs taken from a case occurring on a mountaineer immediately after exposing his perspiring head to the icy wind on reaching the top of a mountain. The first, representing a hair from the periphery of the bald patch, shows a loss of nerve power in the hair preventing it from withstanding the pressure of the root-sheaths by which it is, as it were, cut off, and atrophies at the point of their junction with the hair-root, the newly-formed cells being heaped up below this point and forming a swollen knob. The second plate, a hair after nine days' treatment with tinct. capsici

* Pincus. Deutsche Klinik, Jan. 1869, and Boeck. Virchow's Archiv, xliii. p. 336.

† Archiv f. Dermatologie und Syphilis, 1869, No. 4.

and glycerin, shows the hair preserved by stimulation, pushing onwards again, carrying upwards the fattily degenerated point of atrophy, the part below this smaller than normal, from the pressure of the root-sheaths not as yet wholly resisted, and the bulb much shrunken, its newly-formed cells going to supply the growing hair. Wilson* gives four cases of Alopecia areata following nervous derangement, and adds, "how monstrous it appears to the genuine pathologist to talk of a parasitic fungus in connection with the phenomena now described, or to entertain a mere suspicion of contagion!" and on the next page he speaks of the "heredity" of Alopecia areata as "a sufficient answer to the follies of the fungus theory, the crowning absurdity of a mikrosporon audouini," and cites a case of two young ladies, sisters,† whose uncle also suffers occasionally in a similar manner, while his father before him was afflicted with the same annoyance. Such recurrences of the disease, as well as the relapses during treatment, certainly point to depraved nerve force rather than to spores.

The treatment of Alopecia areata is necessarily confined to nervous and nutritive tonics in general. To these we may add various local stimulants to amuse the patient until the disease chooses to disappear. These are the ethereal oils or stimulating alkaloids dissolved in alcohol. Tincture of aconite, of cantharides or of capsicum, from ten grains to half a drachm, may be used in an ounce of alcohol plus a small amount of some ethereal oil. Or we may employ carbolic acid one drachm, alcohol six ounces, glycerin one ounce, well mixed. Whichever is used, it should be applied twice daily to the bald spots and their immediate neighborhood by means of a stiff, small brush, and the applica-

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* Journal of Cutaneous Medicine, vol. iii., No. 9, p. 100.
† Now under treatment, April, 1869.

tion be discontinued for a time or used more rarely and less violently, if too great reaction is occasioned. The length of time necessary for a cure requires for the patient's and variety's sake, some change from time to time in the drug employed, but do not attribute the cure, when it occurs, to the drug which happened to be the last one used. The loose hairs around the bald patch must be daily removed with hair forceps, after washing the head, before applying the remedies. There is one more idiopathic form of baldness, viz., that occurring in the region supplied by the peripheral branches of a nerve which has been cut or wounded or become in any way diseased, and this form, by analogy, strengthens our conviction in regard to the origin of Alopecia areata. We come now to Alopecia præmatura symptomatica, arising from seborrhœa capillitii resulting generally from chlorosis and anæmia.

For a full account of this final subdivision of our subject, I must refer you to the original article of Dr. Kohn.* Under this head he places baldness symptomatic of disease of the cutaneous tissues or organs, especially of the hair follicles and sebaceous glands, and dependent upon these for its form, extent, duration, intensity, curability, &c. The hairs fall because, by purulent infiltration of the root-sheaths and of the cells of the root, the root-sheaths become separated from the hair and the hair from its papilla. Until the follicles have been destroyed or cicatrized this baldness is curable. Single hairs or groups fall in this way in cases of pustular, lichenous and parasitic diseases. A more extended loss results from diffused inflammatory processes, such as eczema, erysipelas, &c. Such loss requires the protracted existence of the inflammatory condition, inasmuch as the hairs fall in

* Handbuch d. Pathologie u. Therapie, vol. iii., part 2d, p. 156.

the same way that the whole epidermal layer may be thrown off after a chronic serous exudation. When the inflammation is superficial and transient, the patient may experience no loss of hair. Baldness thus occurring is usually only transient. So also the baldness consequent upon fevers, puerperal processes, great loss of blood, &c., the hair returning with the returning strength. Such loss of hair may be due directly to the modified general nutrition, i. e., dependent upon a trophoneurosis, for, in the experiments upon animals of Boussingault* and Magendie,† insufficient or unsuitable nutrition was followed in many cases by loss of hair. But since in such convalescences, in anæmia, chlorosis, &c., the loss of hair is preceded by seborrhœa, and since this is the most common cause of baldness, it seems fair to attribute the baldness here also to seborrhœa as its immediate cause, though this seborrhœa must needs be referred again to the general disturbance in the nutrition. So the baldness in cases of cancer, tuberculosis, &c., is due to a seborrhœa of the scalp, which is represented on the rest of the body by the so-called Pityriasis tabescentium [Hebra].

To premature baldness as the result of seborrhœa, Kohn applies the title of Alopecia furfuracea. At first, the seborrhœa alone is present, the well-known dandruff, easily removed by soap and water, but returning in a few hours; varying in extent and intensity; producing slight itching; lasting months or years; and by preference selecting the top of the head, possibly because this, being farthest removed from the face, is not reached by the daily ablutions. Dandruff is generally present as a symptom of chlorosis in men or women. The diminished vitality expresses itself also "in cold hands and feet, cold perspiration on the palms and

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* Chimie Agricole, Paris, 1854, p. 271.

† Physiologie, Paris, 4th edit., tom. ii. p. 505.

soles, a bluish-red and cold end of the nose, a pale and dry skin and a tendency to chilblains and chronic dyspepsia. In women, deranged menstruation, sterility, pregnancy and puerperal conditions predispose to chlorosis and consequently to dandruff." It generally appears, therefore, at from twenty to thirty years of age, and, though it may last for years, it rarely makes its original appearance after the age of forty. Dandruff is so light an evil that the help of a physician is rarely resorted to until the falling out of the hair and approaching baldness arouse the fears of the patient. The first thing noticed is the great number of hairs lost in combing, and even during the day without combing, until, in from two to six years, the hair has become very thin and often in some places bald. This occurs especially along the part of the hair, on the crown, where the greatest amount of traction is exerted in combing and brushing. The hair on the forehead is not, as the rule, so soon lost, although the baldness may begin here at the same time as upon the crown. This latter is especially common in men, and due perhaps to the pressure and heat of the hat, an ornament which "independent Americans" are rather too fond of retaining upon their heads in season and out of season. Hatters tell me that, according as the head is long or wide, baldness commences at the forehead or on the crown. The perspiration, also, caused by an ill-ventilated hat, is particularly bad for the hair. In very young children we see the hair rubbed off by the pillow; in adults, long-continued pressure may even destroy the follicles. So, according to Virchow, "frequent stimulation, as, for instance, with alcohol, tends to wear out the power of organs by changes in their molecules." The place of the lost hairs is supplied for years by small lanugo hairs, which finally also disappear. The chief cause of premature baldness, then, in both men and women is sebor-

rhœa, or, in plain English, dirt, a lack of cleanliness and proper care of the hair; the superstition being widely spread that washing, especially with soap, is injurious to the hair and skin. A more frequent and thorough use of the matutinal bath would conduce to more health and less baldness among our free and enlightened citizens.

The explanation of the pathological process resulting in baldness is simple. Seborrhœa is the result of an excessive proliferation of cells from the sebaceous follicles which have undergone "fatty impregnation, an organic chemical change." The cells of the external sheath of the hair-root corresponding to the rete mucosum, form, by an extension in continuo, the lining also of the sebaceous glands. When now the cells of the sebaceous follicles become diseased and are thrown off in excess, the same process is continued in the extension of these cells, viz., the external sheath of the hair-root, and the hair thus separated and pushed out falls as a matter of course, the change in nutrition producing naturally a mechanical separation. The gradual progression of the process and its nature are evident from an examination of the hairs which fall daily. Hairs consist of two classes: long hairs, living from two to four years before they fall naturally; and short hairs, existing four to nine months. "Normally, the proportionate loss of short hairs to that of long ones is from one in eighteen to one in fourteen. As the disease progresses, the proportion rises to one in eight, or in the later stages to one in two," and at first the absolute loss of hair is little if at all increased, merely the relatively proportionate loss of short to long hairs. This would simply tend to show that the hairs were extruded before their time. As the disease increases, however, the absolute loss of hairs is increased, while their size constantly diminishes from progressive debility of reproductive power till we find only lanugo hairs and

finally not even these. This complete loss of hair requires from six to ten years, and the baldness is now incurable, the papillæ and their vessels being so atrophied as to be incapable of producing new cells for young hair-bulbs or roots."* Should we, however, by treatment, put an end to this hyperplasia of cells before the atrophy of the papillæ is complete, the process is arrested and new hairs may be again formed, which, with the restoration of the papillæ, will approximate more and more to their normal condition.

Baldness following variola may be due to destruction by suppuration of the walls of the sacs and of the sheaths of the roots, and in these cases is incurable. But it may also be due to a seborrhœa, when the hairs have escaped destruction, the variolous process not having penetrated so deeply into the corium. Here, after the expiration of the variolous process, there may occur a disease of the sebaceous follicles described by Hebra as seborrhœa congestiva, a condition which may go on to the development in places of a Lupus erethematosus. Clinical observation and microscopical investigation show here a cellular infiltration of the papillæ surrounding the sebaceous and hair follicles, a sort of chronic inflammation. This seborrhœa in time changes its appearance to that of a Seborrhœa sicca, or, as Kohn calls it, furfuracea, with subsequent baldness just as if from an idiopathic seborrhœa not due to variola.

Baldness resulting from syphilis has long been recognized, being mentioned by no less than ten writers in the Aphodisiacus.† Fracastor also speaks of the "ridiculous" appearance made by such sufferers, and goes on to show that treatment by mercury has nothing to do with the origin of the disease. This baldness, when the result of the destruc-

* Bisiadecki. Stricker's Handbuch d. Lehre v. d. Geweben, iii. Lief. p. 602.

† Edit. repurgata, MDCCCXXVIII., tom. prim. p. 714, C. et seq.

tion of the hair follicles by ulceration with subsequent cicatrization, is incurable, as in the same sort of cases in variola. This baldness is necessarily local, being confined to the spots of ulceration. But syphilitic baldness proper, diffused and uniform, is due to seborrhœa, the same as any non-specific baldness, resulting from any other depravation of vitality, whether this be due to disease, to abuse of stimulants or narcotics, to excess of venery, to onanism, or to any other cause whatsoever.* The seborrhœa may accompany the earlier constitutional symptoms, or occur at any subsequent period, even, in fact, after all other symptoms have disappeared. It begins as S. oleosa, and only after some time assumes the character of S. sicca, but may then, as such, last for months or years, like a non-syphilitic seborrhœa, producing the same effects and in the same localities. In seborrhœa from syphilis, the crusts are yellow, and from their under surfaces little processes dip down into the sebaceous glands. The baldness, however, is more rapid in its course, just as the development of the seborrhœa is here more acute and intense. I trust there is no need of saying here that, since the alopecia occurs after any sort of treatment or none at all, the use of mercury has nothing to do with producing it. And in general, beyond temporary salivation and trembling of the limbs, the terrible constitutional effects of mercury may be regarded as Bogy stories to frighten children. The cure of the baldness lies in the cure of the seborrhœa, and even after four to six years of Alopecia furfuracea, complete or partial restoration of the hair is often possible; in fact, wherever the follicles are not destroyed or cicatrized, and where the cause was a disease of short duration and comparatively slight intensity.

* For nerve lesions in syphilis, compare Fournier, Lagneau fils, Ladreit de Lacharriére, Gros, Zambaco, et al.

The treatment of seborrhœa should be both local and general, and everything depends upon the manner in which the remedies are employed. There is no class of diseases, says Hebra, wherein, to such an extent as in skin diseases, the success or failure of the physician is solely due to the manner of employment of the remedies. With the same patient and the same remedies one physician will be nearly always successful, another will nearly always fail. Some minuteness of description in regard to the proper method of employing the remedies for Alopecia furfuracea is therefore, I trust, excusable. The crusts are to be softened with olive oil rubbed on every two or three hours with a small sponge or piece of flannel; at night, in addition, the oil should be poured on the head, if the crusts are thick, and the whole covered with a flannel night-cap. In twenty-four hours, the crusts will be softened. They should then be washed away thoroughly with soft soap and a flannel rag. Better still is Hebra's alkaline alcoholic soap, composed of two parts of German potash soap [sapo viridis], and one part alcohol, filtered after standing for twenty-four hours. A little spirits of lavender may be added to suit the taste. This is to be poured on flannel or on a bathing mitten, and the head thoroughly washed, the cloth being dipped from time to time in luke-warm water so as to produce a foam, the soap otherwise hardening as the alcohol evaporates. After the scales and crusts are entirely removed, cold or luke-warm water should be allowed to run over the head until every trace of the soap is removed. For this purpose a cold douche is best. This whole washing is best performed in a hot or steam-bath. The oil applications should be daily repeated as long as crusts tend to form. The washing and the cold douche should be continued for weeks afterwards, preferably at bed-time. After washing, the hair should be well combed

and all scales removed, never combing so as to touch the skin of the scalp. The hair, especially of women, may then be dried with a soft towel and left free all night. During the first few days the fall of hair will be greater than usual, but these hairs were already dead and ready to fall, and if not washed out would have fallen of themselves in a few days. If preferred, brandy or alcohol may be substituted subsequently for the alcoholic soap; the last is, however, preferable, and is often of itself alone sufficient to cure the disease. But the epidermis loses its fat from the application of the soap or alcohol, and may scale off as a pityriasis. This is easily obviated by rubbing in some pomade after the hair has become thoroughly dry.

All that remains to be done, after the cure of the disease, is to gently stimulate the growth of the hair. This is never to be done by cutting. No disease of the hair or skin requires for its treatment the cutting of the hair, a barbarous custom, which does not increase the number of individual hairs, which mutilates the patient for the time-being, and which prevents the naturally long hair of young ladies from ever attaining again its usual length. True, the hair is stimulated to an unnatural precocity of growth as regards length; that is, the cut hair plus all the clippings exceeds, at the time of death of the hair, what would have been its natural length; but at the same time the life of the hair is not prolonged, wherefore at the time of its natural death and fall it is still shorter than it would have been had it never been cut; in other words, the excess of growth is not sufficient to supply the actual loss in length produced by the clipping. The stimulation of the hair is best effected by moderate irritants, astringents and tonics, such as veratria, cantharides (tincture), tannin, quinine, &c., never employed to such an extent as to produce inflammation or eczema.

To these may be added alcohol or ether, which stimulate and at the same time, by the chill produced by their evaporation, act as astringents. To avoid two separate applications, it is best to add these drugs to the pomade, to the use of, and necessity for which, we have just alluded. Avoid oil of savine, which turns the hair a reddish brown ; and bicarbonate of soda, which renders the hairs brittle.

So much for the baldness caused by seborrhœa and cured by its removal. But for a permanent cure we must look to the cause of the cause, to wit, the chlorosis and anæmia, no matter from what cause proceeding, which give rise to the seborrhœa as their consequence. The treatment of these needs, of course, no description. Attention to bathing, fresh air, and exercise ; regular hours [and these early ones] ; suitable diet, such as meat and eggs, which tend to form horn ; and sulphur or manganese, according as the hair is blond or dark ; and proper clothing, as flannel next the skin, &c. ; tonics, as iron in a form adapted to the idiosyncrasies of the patient ; and sometimes, though rarely, that much-abused remedy arsenic, which, as a rule, is adapted to but very few cases of skin-disease, will often prove sufficient to overcome the evil. Dyspepsia or chronic gastritis* is often present, and these must be regarded. The iron should be given for from four to six months at least, and a visit to the sea-shore in summer, with sea-bathing indulged in if possible. For baldness following the variolous process, local treatment will generally be found sufficient. For the baldness of syphilis, in addition to the usual local applications, it is well to employ an ointment consisting of a drachm of white precipitate to an ounce of fat. Otherwise, no general treatment for syphilis unless other appearances of syphilis

* Sodæ bicarb., Magnes. carb., Sacchari alb., aa p. æq. ½ dr. ter die for weeks.

are present. Where the loss of hair is dependent upon a local inflammatory process, e. g., from ulceration, eczema, or parasites, &c., our treatment should be wholly directed against these conditions. Microscopical examination of the hairs lost daily gives us a good standard for therapeutical agencies.*

The nerve system affected is the sympathetic. It occurred to me that the motor and sensitive systems might be implicated, as baldness prefers the crown over the aponeurosis, where anatomically we should expect less nervous distribution. But experiments showed that the sensitiveness of the crown does not greatly vary from that of the parietal regions lying over muscles; and the motor nerves of the two regions responded in equal degrees through the Arrectores pilorum to the stimulus of electricity in the case of a boy placed upon an insulated stool and electrized by means of a battery. The loyalty of alopecia to the crown must, then, be referred to some other cause than special deficiency of local nerve force.

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* Pfaff. Das menschlicke Haar, Leipzig, 1869, p. 57, and Pincus. Deutsche Klinik, 1871, I, Lfg.; also Central-blatt f. d. Med. Wissenschaften, April 1st, 1871.

THE

THEORY OF TUBERCULOSIS.

A BRIEF ACCOUNT

OF

SOME OF ITS MORE IMPORTANT FEATURES.

By R. H. FITZ, M.D.,

OF BOSTON.

READ JUNE 6, 1871.

THE THEORY OF TUBERCULOSIS.

As is well known, the theory of tuberculosis at the present day differs widely from that found in the majority of text-books, and the results furnished by recent investigation and experiments have not as yet met with that general attention which they deserve. It is not my purpose to bring forward matter resulting from original research on my own part, but to lay before you facts derived from the investigations of others, together with some of the conclusions which have been drawn, especially with reference to the relation which phthisis bears to tuberculosis.

The term tubercle was originally adopted for the purpose of describing the shape and size without reference to origin or structure, hence was not suggestive of a specific condition. Tubercles were small, more or less spherical nodules, at first seen upon the surface of the body, then upon bones; later, as anatomy demanded an increased field for observation, little nodules were also found in the interior of the body. The specific tubercles were first spoken of towards the close of the last and at the commencement of the present century.

The term phthisis too, originally referred simply to the wasting occurring in certain forms of chronic disease; it was used merely in the clinical sense, without special reference to anatomical conditions. The most common form of phthisis being that arising from chronic disease of the lungs, its use

became more or less restricted to these organs. The term phthisis pulmonum, even in ancient times, by no means referred to a single form of disease. The history of this subject has been most carefully and thoroughly developed, in most cases from the original sources by Waldenburg,* of Berlin.

Sylvius† described a condition of the lungs where ulcers and abscesses existed, either directly or from metamorphosed blood, and another in which larger and smaller tubercles were present, which might soften and thus give rise to cavities. He regarded these tubercles as glands which enlarged in the same manner as scrofulous glands elsewhere in the body. From this point of departure arose the confusion between phthisis and scrofula.

During the following hundred years, similar ideas prevailed. Mead‡ says, "And indeed in the dissection of bodies dead of consumption we very often find the lungs beset with tubercles or indurated glands which had suppurated and thrown off purulent matter." Cullen§ considered phthisis pulmonum to be "an expectoration of pus or purulent matter from the lungs, attended with a hectic fever." In all cases of the expectoration of pus he assumes that an ulceration of the lungs exists, which ulcer may arise from hæmoptysis, from suppuration the result of pneumonia, from catarrh, asthma and tubercle; tubercle he considers as the most frequent cause. Tubercles with him mean "certain small tumors which have the appearance of indurated glands;" these, indolent at first, become inflamed and thereby changed

* Waldenburg. Die Tuberculose, die Lungen schwindsucht und Scrofulose. Berlin, 1869.

† Waldenburg, op. cit., p. 27.

‡ Mead, The Medical Works of, Edinburgh, 1775, p. 358.

§ Cullen, The Works of, Thomson, Edinburgh, 1827. Vol. II. Book 4, chap. 4.

into little abscesses or vomicæ which break into the bronchi. He admits that it can hardly be supposed that all the tubercles are tumefied glands, and accounts for some through hæmorrhage into the cellular texture of the lungs, the entrance of dust, which obstructs the bronchi, and either thus form cysts or produce congestion of the neighboring glands which have the appearance of tubercle.

A year after Cullen had thus declared his views (1784), Stark, an English physician, first described at length the small tubercles, and soon after, Reid* separated tuberculosis from scrofula, and regarded the tubercle as something quite distinct from a gland and denied its origin from hæmoptysis.

Baillie† accepts the new idea and denies the glandular structure of tubercles. "They are at first very small, being not larger than the heads of very small pins, and in this case are frequently accumulated in small clusters. The smaller tubercles of a cluster probably grow together and form one larger tubercle. * * * * * When cut into, they are found to consist of a white, smooth substance, having great firmness and often containing in part a thick, curdly pus." Baillie considered tubercles as deposited scrofulous material.

In 1803, Vetter‡ distinguished a phthisis pulmonum resulting from vomicæ, the result of inflammation of the lung tissue, and a tabes pulmonum caused by tubercle. He agreed with Reid in that the tubercle had no necessary connection with scrofula nor with glands. He granted that the tuberculous material resembled that which one found in

* Reid. An Essay on the Nature and Cause of Phthisis Pulmonalis, London, 1785, p. 37.

† Baillie.. The Morbid Anatomy of some of the most important parts of the Human Body, London, 1793, p. 46.

‡ Waldenburg, op. cit., p. 57.

scrofulous glands, and called it cheese-like. He described tubercles of the peritoneum, intestine, liver, spleen and uterus, but did not connect them with tubercles of the lung; the latter were specific, and had nothing in common with the former.

The great work of establishing the theory of specific tubercle was now transferred to Paris, and Laennec's brilliant discovery enabled him to present his theoretical views in an exceedingly practical light.

The new movement was excited by Bayle,* however. Finding similar appearances in other organs in cases of pulmonary consumption, he considered that such might result from a common cause. With him, tubercle became tuberculous matter, the tuberculous affection was probably of a scrofulous nature, and the scrofulous glands were those which were transformed, entire or in part, into tuberculous matter. Bayle† also described the miliary granulations; these were never opaque, nor did they soften, and were thus to be distinguished from miliary tubercles which they very much resembled in size, but which, on the contrary, were always gray or white and opaque. These miliary granulations also occurred in various organs and produced a specific general degeneration. The miliary granulations and the miliary tubercles generally occurred simultaneously, yet, on account of the differences previously mentioned, were considered as having a different origin, though each produced a special variety of phthisis.

Laennec‡ now (1819) published his work on auscultation, in which he brought forward his theory of tuberculosis,

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* Bayle. Recherches sur la Phthisie Pulmonaire. Paris, 1810.

† Op. cit., p. 26.

‡ Laennec. Traité de L'Auscultation Médiate et des Maladies des Poumons et du Cœur. Tomes II., 2ème Edition. Paris, 1826.

the same which was accepted by the general medical world, the same which in the minds of some exists unaltered at the present time. The tubercular material* could appear in the lungs and in other organs, as isolated bodies and as infiltrations; of the former were miliary tubercle, crude tubercle, tuberculous granulations and encysted tubercles. Tuberculous infiltrations occurred as gray and yellow shapeless masses.

The tuberculous material originally is gray and semi-transparent, later it becomes yellow, opaque and very dense. Then softening occurs, and one has a fluid resembling pus; this being expelled through the bronchi, there remains the tuberculous cavity. Phthisis is not the result of an inflammation of the lungs, a view already advanced by Bayle. Tubercles are not caused by hæmoptysis, though the latter often occurs when the former are present. Laennec describes the various organs where tubercles are found; he states that they may also exist in certain tumors which one generally confounds under the term scirrhus or cancer; in such cases they may be either isolated or diffused. The development of tubercles is the result of a general disposition, and if inflammation occurs in connection it is generally secondary.

With Laennec, then, there was but one pulmonary phthisis, and this the result of the development of a specific growth, the tubercle. All nodules in the lungs, excepting those which were cancerous or cretified, whether varying in color, size or density, were tubercles. Nearly all masses filling up the air spaces, in long-standing disease, came under the same general law, and all cavities were the result of the same original cause, with the exception of those arising from gangrene of the lung.

* Op. cit., vol. i. p. 532.

Broussais,* the most active opponent of Laennec, published the first edition of his work in 1808. Phthisis† with him was a chronic inflammation destroying the lungs, almost always produced by a degeneration of the lymphatic vessels. Pneumonia, catarrh and pleurisy could produce tubercular phthisis. In inflammation of the lymphatic glands‡ one has a gray, brawny swelling; instead of an exudation, one has a white, concrete, inodorous product, offering exactly the aspect and consistency of cheese. One has agreed to call this white matter tubercular.

The views both of Broussais and Laennec contained each the germs of truth, as we shall subsequently see; but in order to carry out individual theories, facts were made to correspond with theories, rather than the reverse, and as Laennec's ideas prevailed, the truths of Broussais remained dormant.

Of the two celebrated authors who followed Laennec, the one, Andral,§ differed somewhat in his views. Tubercle was considered as the result of a modification or perversion of secretion, often attended or preceded by an active or sanguineous congestion. This secretion was most usually seated in the cellular tissue, hence all tubercular matter is infiltrated. The original form of the tubercle was not "a grayish, semi-transparent granule."‖ In all the granulations tuberculous matter may be formed as pus may; the granulations are for the most part the effect of a partial pneumonia. From the views thus stated, it would seem that he followed

* Broussais. Histoire des Phlegmasies ou Inflammation chronique. 2 Tomes. Paris, 1808.

† Op. cit., vol. i. p. 318.

‡ Op. cit., vol. i. p. 21.

§ Andral. A Treatise on Path. Anatomy, translated by Townsend and West. 2 Vols. Dublin, 1831.

‖ Op. cit., vol. i. p. 509.

Broussais in considering the tubercles as the result of inflammation, but differed from him in not confining the same to the lymphatic system. From Laennec he differed in denying the specific character of the tubercle, and in considering the miliary granulations as having nothing in common with tubercle. Andral made a very valuable contribution to the subject by calling attention to the condition of the lymphatics leading from intestinal ulcers, which he found to be laden with pus in a fluid or semi-solid condition, bearing, as he says, a strong resemblance to the matter of tubercle. He also speaks of the presence of an apparently tuberculous matter in the interior of cavities lined by a mucous membrane, where ulceration has not existed, and states that mucous follicles are sometimes filled with a substance which has exactly the aspect of tuberculous matter.

The other author, whose influence was exerted in favor of Laennec, was Louis.* The tubercle was specific, the primary stage was that of the granulation, through which the tubercle must pass before assuming its peculiar character, i. e. becoming dull, yellowish-white, of friable consistency. At the same time he suggested that in certain cases of acute phthisis the tuberculous matter might be deposited in its mature, characteristic state, from the fact that sufficient time had hardly intervened for the transformation of an originally gray matter.

In Germany, but little original thought on the subject of phthisis had arisen, till these ideas of Laennec and Broussais were promulgated.

Schönlein† makes a distinction between scrofula and tu-

* Louis. Pathological Researches on Phthisis. Cowan's Translation. London, 1835.

† Schönlein. Allgem. und Specielle Pathologie und Therapie. 4 Theilen, 3te Auflage. St. Gallen, 1841.

bercle. In the former,* a peculiar diseased product arises, of the density of Swiss cheese, a brittle, yellowish-white mass; this is the scrofula-matter, which may be deposited in various organs. The tubercle is distinguished from the same as being a true new-formation, round. He states that tubercle† is allied to scrofula, though not identical; commencing as a vesicle filled with gelatinous fluid, the agglomeration of such and the individual increase in size produce varieties of form. Pulmonary phthisis not rarely occurs from other causes than tubercles. In phthisis‡ a morbid surface forms in the diseased organ, from which a peculiar stuff is secreted, which one generally calls pus; as this secretion becomes more copious, one has consumption. A similar formation in other organs than the lungs produces phthisis also; e. g. phthisis hepatica, a term applied to abscess of the liver.

Rokitansky§ considered tubercle as an exudation of solidified protein substances (fibrine, albumen), distinguished by the tubercular form, either as scattered or collected nodules, or by its deposition in granulations and stellate masses. This view resembles very much that of Andral, his secretion being Rokitansky's exudation; the latter, however, made a decided advance, in re-directing attention to the descriptive element of the term. He admitted the gray, semi-transparent tubercle, but also a "fibrino-croupous" form which was opaque from the first. These two forms were distinguished from one another also by final results, the former shrivelling into a tough, amorphous or distinctly fibrous

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* Op. cit., 3te Theil. p. 44.

† Page 69.

‡ Page 89.

§ Rokitansky. A Manual of Path. Anatomy, 4 vols. Sydenham Edition, 1852. Vol. i. p. 293.

mass, the latter softening and producing ulceration. Tubercle was the product of inflammation; a portion of the resulting exudation, however, might become absorbed or organized, or the entire exudation becomes tuberculous. Tubercle and scrofula are identical; tuberculosis and scrofulosis one and the same disease. He also maintained the existence of another variety of tubercle, the albuminous, occurring in acute tuberculosis. This form is only rarely the primitive one, and is subject to no metamorphosis.

Vogel's* researches threw additional light upon the subject. He states that in scrofulosis as in typhus, depositions occur in various parts of the body, most commonly in the lymphatic glands and their vicinity, but also in other glands and in other organs; the only essential difference is, that the whole proceeding is accomplished much more slowly, the deposit and softening generally lasting as many weeks or even months, as in the other case days. Scrofulous matter cannot with certainty be distinguished from typhoid or tuberculous matter. Tubercle is a pathological new-formation, produced in consequence of a specific disease, or morbid tendency, tuberculosis. Tubercular matter cannot be distinguished with certainty from the scrofulous, typhous, or from that derived from any other ulcerative process. The formative substance of tubercle is originally fluid, secreted from the capillary vessels, the results of the blastema theory, the view entertained by Schönlein. Truthfully enough he states that this fluid condition cannot be directly observed, for one first sees the tubercles as solid bodies. Gray and yellow tubercle can both exist primarily. In the former, we have an amorphous mass and cellular structures, in the latter the granular elements prevail.

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* Vogel. The Path. Anatomy of the Human Body, translated by Day. London, 1847.

Lebert* regarded as peculiar to tubercle, certain bodies whose characteristics are very completely described in the article referred to. By means of the microscope he asserted that he could distinguish the tubercle from pus or cancer, and by the microscope alone can one distinguish between concrete pus and tubercle. These corpuscles were seen best in the yellow tubercle, though they also exist in the gray form. There is no peculiar scrofula material; what one regards as such, is generally the result of an ordinary inflammation or suppuration under the influence of a dyscrasial element. These views with regard to the structure of tubercle were generally received till 1847, when Reinhardt and Virchow† published the results of their investigations, the one with regard to the granular corpuscles, the other with regard to the development of cancer. It was there shown that these tubercle-corpuscles were shrivelled cells and nuclei in a stage of retrogade metamorphosis, and were to all intents and purposes dead. The metamorphosis was shown in physiological and pathological conditions of the most varied nature. Virchow called attention to the fact that the masses thus formed in cancer bore the greatest resemblance to crude tubercles, the same with regard to inspissated pus. Some years later one of our own number, Dr. C. Ellis,‡ from a series of special investigations, extending over a number of years, came quite independently to a similar conclusion, that the yellow tubercle was simply the result of degeneration.

In 1850, Reinhardt§ asserted that the changes taking place in tubercular processes agreed with those occurring in chronic inflammation; "every deposition of tubercle is

* Lebert. Müller's Archiv für Anatomie und Physiologic, 1844, p. 194.

† Reinhardt. Virchow. Archiv für Path. Anatomie, 1847. Vol. i. pp. 20, 94

‡ Ellis. American Journal of the Med. Sciences, 1860.

§ Reinhardt. Annalen des Charité-Krankenhauses. Berlin, 1850.

only a more or less extensive chronic pneumonia, and the only peculiarity of pulmonary tuberculosis is, that these chronic inflammations repeat themselves at longer or shorter intervals."* Reinhardt therefore agreed more particularly with Broussais and Andral, and denied *in toto* the views of Laennec with regard to the specific character of tubercle.

Virchow, starting with the same amount of positive knowledge of the condition of so-called yellow tubercle, came to different results. The error of Reinhardt was the same as that of the observers preceding him. A given series of facts being obtained, all conditions were regarded as holding a necessary connection with the single premises. Virchow saw that the yellow, opaque material resulted from various conditions in different organs, and under circumstances which had no connection with phthisis or tuberculosis, as well as the reverse, and instead of seeking at the outset for a common pathological cause, inflammation for instance, the history of each individual process resulting in the formation of the so-called tuberculous or scrofulous material was sought for, and the result was arrived at that there were inflammatory conditions producing the result, and that there was a specific tuberculosis in which, at a certain period, similar appearances presented themselves.

In 1850,† speaking of tuberculous inflammation, he calls attention to the fact that in the inflammation of serous membranes the fibrinous exudation becomes organized, at the same time tubercles may form. He opposes the idea of Rokitansky that a mixed exudation of inflammatory and tuberculous constituents existed; on the contrary a vascular cellular tissue formed in one part, while in another, the formation of cells increased by endogenous growth so rapidly

* L. c., p. 376.
† Virchow. Wurzburger Verhandlungen, 1850. Vol. i. p. 83.

that their number became very great at certain points. The retrograde change then commenced; after partial fatty metamorphosis, the cells became destroyed and a granular detritus remained in which the cell-nuclei could be recognized for some time. The tubercle here arises from a metamorphosis of organized elements, not from an exudation. The so-called tubercle corpuscles are the altered nuclei. He then calls attention to the fact that the tuberculoid (tuberkelartige) metamorphosis consists in an atrophy of the tissue elements, accompanied with inspissation. The commencement of the process is the origin, accumulation and endogenous increase of the nucleoid and cell-like formations. He acknowledges that it is impossible to state whether this process is inflammatory or not. "We know that inflammatory new-formations may become tubercular, and that the tuberculoid metamorphosis is not the peculiarity of a specific process, of a special constitution." The distinction here drawn between the tubercular process and the tuberculoid metamorphosis is the starting point, the one which is the groundwork of the present theory, the one which is totally at variance with that of Laennec, for with him tubercular and tuberculoid were synonymous, the latter being merely a stage of the former.

Virchow also, in the article referred to, contends against the dyscrasial nature of tuberculosis, as one has not been able to find any special alteration of the blood.

The tuberculosis is a local process, in many cases produced by merely local conditions; that the process may afterwards become "constitutional" is not denied. He also calls attention to the fact that certain organs are predisposed to tuberculosis. At this time it seems as if a distinction were made by him between tuberculous inflammation and specific tuberculosis, though the development is asserted as

analogous. In speaking of specific tuberculosis the lungs are referred to as the example, and the development of tubercle is to be seen to the best advantage in the epithelium of the pulmonary alveoli. First the endogenous formation of cells, then the metamorphosis either fatty or tuberculoid. Then again,* with regard to the formation of cavities in the lungs, he states that such are simple bronchial dilatations, ulcerative cavities, the result of tubercle, abscess, gangrene, or mixed forms of dilatation and ulceration. The pulmonary tubercle then, so far as one can judge, is not only a mass of tuberculoid metamorphosis, but also the single specific tubercle of Laennec arising from changes in the pulmonary epithelium.

In 1851† he expresses his views with more precision:

"1. Tuberculization, the undoubted local process through which the body called tubercle is formed, does not consist in a peculiar specific exudation, but in a peculiar conversion of tissue elements, which in 1847, as occurring in cancer, I described under the name of tuberculoid metamorphosis.

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"8. Tuberculosis is the entire process of the disease which comprises the conditions of local nutritive disturbances with the accompanying changes of exudation, cell-formation, and metamorphosis, and which finds its regular expression in tuberculization. Every tuberculization (tuberculoid metamorphosis) does not proceed from tuberculosis. Tuberculosis may be present in its earlier stages (exudation, cell-formation) even when no tubercle is present. We call tuberculosis the diseased process which in its usual course always leads to tuberculization, while we refer to an entirely different process, cancer and sarcoma, which accidentally become

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* Würzburger Verhandlungen, vol. 2, p. 27.
† Würzburger Verhandlungen, vol. 2, p. 72.

tuberculoid, and the term tubercle should never be applied to a condensed abscess, cheesy pus (*pus concret*).

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"10. The tubercle, in so far as it arises from the accumulation of cells in tissues of the most varied sort, which cells in the majority of cases fall to pieces, is destitute of every complete, really characteristic element. Of the remains of the cells the shrivelled nucleus presents the most constancy in its external appearance, and therefore we may retain for this the name of tubercle corpuscle."

From this statement one would infer that the tubercle first existed after tuberculization (the tuberculoid metamorphosis) had commenced, though in tuberculosis a stage existed where no tubercle was present. That confusion should arise from such a collection of terms was a necessary result, and Lebert suggested the name "phymatoid" for the tuberculoid condition.

In 1852,* the statement of the distinctions between phthisis and tuberculosis pulmonum is made, and at the same time the terms are so altered that misunderstanding is prevented. The general medical world received for the first time a systematic collective representative of Virchow's views upon the subject. The term "cheesy metamorphosis" is substituted for the tuberculoid; the tubercle is a peculiar, nodular, organized formation which under certain conditions may become cheesy. After calling attention to the fact that since the time of Laennec one has identified pulmonary phthisis with tuberculosis, he states that his own views with regard to pulmonary phthisis are more allied to those of Bayle and even Morton. He finds various sorts of phthisis which depend upon pulmonary affections. The cheesy infil-

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* Würzburger Verhandlungen, vol. iii. p. 98.

tration of the pulmonary parenchyma is not a necessary condition of phthisis, whether it is in connection with real tubercle or with a condensation of accumulated bronchial secretion. Ulcerative bronchitis, chronic, broncho-blennorrhœa, with cheesy infiltration of the bronchial glands, purulent chronic pneumonic infiltrations, all produce phthisis. The cheesy masses which Carswell and Reinhardt found in the bronchi and alveoli of the lungs, and whose development was traced from thickened pus, are not tuberculous, for at times near them or independent of them one sees the same nodular disease which is so pronounced in tubercular meningitis. Acute and chronic tuberculosis are developed from gray, cellular, at first soft, later firm accumulations, in which the brittleness of the cells and the abundance of the nuclei are striking. There is a tubercular bronchitis in which the membrane secretes pus and contains in its interior tubercles, just as in a tubercular meningitis one has tubercular granulations and fibro-purulent infiltration. He considers it advantageous to separate altogether the idea of pulmonary tuberculosis from pulmonary phthisis.

Virchow's views thus set forth were productive of immense results. Other workers entered the field, and the various questions which arose as to the origin, contagiousness, inoculation and treatment are still in the process of solution. What tubercle is, seems definitely settled through Virchow's means; how and why it occurs are debatable questions, the weight of evidence tending now in this direction, now in that. The diagnosis of tubercle in special instances is a matter of some considerable uncertainty. Indeed it is by no means improbable that certain appearances which one now calls tubercle, at some future day may be regarded as having only an accidental connection; the general tuberculosis which so often coëxists may be simply consequent with-

out being specifically so, though a causal relation may very likely exist.

Since 1852, with the increased field for observation and the valuable assistance to be derived from experiment, with improvements in method and instruments, the views of Virchow have become elaborated and generalized, till the theory of tuberculosis has become a statement of facts explained in a manner which leaves but little room for doubt. Virchow's views are to be found most fully stated in his work on Tumors,* a work unfortunately not yet complete.

What then shall one consider as tubercle, and how is the same to be distinguished from other appearances which have hitherto been regarded as such? According to Virchow, the tubercle is an organized new-formation in that it has a cellular composition, though non-vascular. He classes it among the tumors, considering it as a sort of lymphatic tumor from the structural resemblance to that of the lymphatic glands. It is a small, spherical body, about the size of a Malpighian corpuscle of the spleen, often, indeed, somewhat smaller as in the acute tubercle of the liver, and rarely larger than a mustard seed. At first gray and somewhat glistening, as it becomes older the fatty degeneration going on in its cellular elements gives rise to a white, opaque appearance, with less lustre, and finally the nodule becomes yellow.

As before stated, it is made up from the beginning of cells; these are spherical, in general smaller than the white corpuscles of the blood, and contain one or more nuclei. Large cells exist, however, though to a slight extent, and contain several nuclei, at times as many as twelve. These cells are closely packed together in a delicate meshwork of slender fibrils. Should bloodvessels be found in the interior

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* Virchow. Krankhaften-Geschwülste. 3 vols. Berlin, 1863. Vol. ii. p. 620.

of the tubercle, they are, as a rule, such as previously existed in the part before the tubercle made its appearance. The cells have only a brief period of existence, and rapidly undergo fatty degeneration ; hence it is rare to find the tubercle, at least in most organs, where this change has not already commenced. The degeneration begins in the centre, the part farthest removed from nutrition, and is often recognized as an extremely minute white spot in the midst of the otherwise gray, somewhat transparent nodule. At the same time there is more or less peripheral absorption, producing a certain degree of inspissation of the new-growth, and one has the cheesy tubercle. Later, softening, liquefaction occurs, at first in the centre, and on section the minute cavity is seen filled with a yellowish-white and opaque fluid of the consistency of cream. Virchow calls attention to the more fibrous tubercle which developes in firm, fibrous parts, not unfrequently where newly-formed fibrous tissue exists. Such are denser, more transparent and pearly, and are usually found in connection with the more common form.

Tubercular nodules are often found as large as walnuts ; these, however, are never single tubercles, but are made up of hundreds, thousands, even, of the minute, individual tubercles. The growth of these larger nodules takes place from the periphery, and one sees upon the border, in the injected tissue, the small gray tubercles immediately surrounding the large nodule. The agglomeration of such produces the central mass, which latter has already undergone the cheesy degeneration. Not rarely one finds about the central mass a more or less firm capsule of connective tissue in which the growth advances. These tubercular nodules are best seen in the brain and spinal cord. The supra-renal capsule, too, is a frequent seat. When such a change takes place in the kidneys, the tubercles are packed closely to-

gether, imbedded in the structure of the kidney, the epithelium of which undergoes a similar metamorphosis to that of the tubercle cells; and these changes commencing generally in the medullary region, as softening occurs portions are discharged, one has a large, irregular cavity with cheesy walls, communicating freely with the pelvis of the kindey, a condition which has been called renal phthisis. The agglomeration of the single tubercles takes place also on serous membranes, as the pleura and peritoneum; in such cases the form is rather that of flattened, somewhat elevated patches. When the tubercles occur in mucous membranes they are, as always, at first isolated; as the cheesy metamorphosis advances, one has a death of that portion of the membrane containing the tubercle, and this dead portion is thrown off, a minute, relatively superficial ulcer resulting.

In tuberculosis of these membranes, numerous tubercles exist however, numerous ulcers follow, these ulcers run together, and the border becomes serpentine and crenate, owing to the confluence of the various ulcers.

Such changes are best seen upon the mucous membrane of the pelvis of the kidney, the uterus and bladder. Then, too, one often finds secondary tubercles imbedded in the base of the ulcer and in its periphery, so that the ulcerated surface increases in depth and width by the softening and discharge of these, seen to best advantage in tubercular ulcers of the intestine. Virchow speaks of the minute, shallow ulcer of the larynx, where the original tubercles, lying quite superficially, disappear by a loss of substance from the surface, without becoming cheesy.

Then again, in the smaller bronchi, the vasa deferentia and vesiculæ seminales, where a free outlet to the secretion does not readily take place, one has in addition to the cheesy tubercle imbedded in the thickened, white and opaque mu-

cous membrane, a mass of secretion constantly increasing, and undergoing the cheesy metamorphosis; so that finally the tube becomes distended with the mass of softened tubercles and catarrhal secretion from which the fluid portions have become more or less completely absorbed, and on section one sees the firm, yellowish-white, homogeneous mass, presenting the general resemblance to Swiss cheese. Such, then, are the appearances assumed by the tubercles from their perceptible origin till their disappearance through softening or ulceration, or both.

All tubercles are at the outset miliary, that is, minute, and where an appearance of so-called infiltrated tubercle exists it is almost invariably due to other causes, with perhaps but one exception.

In speaking of the conglomerated tuberculous nodule of the brain, Virchow states that a period of rest in the growth of the same may occur. Hence one does not find upon the border, at this stage, the minute tubercular granules, but, on the contrary, one has a continuous, hard, cheesy substance. As one can readily see, the admission of such an occurrence renders extremely difficult in many cases the diagnosis of such a cheesy mass. There are cheesy alterations occurring in syphilitic tumors of the brain, so that from the appearance alone of the new-growth a differential diagnosis is impossible. Virchow states, indeed, that there are cases where one is even inclined to admit a third form of cheesy cerebral tumor. As a rule, however, the clinical history of the case, and the appearances found in other organs, will enable one to form a relatively accurate opinion.

Where, then, does the tubercle exist? According to Virchow, in the connective tissue or in allied tissues, as the medullary, fatty, and osseous, and arises from a proliferation of the pre-existing tissue, not from an exudation. The con-

nective tissue, however, may have arisen from an exudation, as in tubercle of pleuritic adhesions. In the brain one finds the minute tubercles in the external sheath of the smaller arteries of the pia mater. Virchow speaks of an appearance often seen in connection with the dense fibrous form of tubercle, where the suspicion arises that in them lymph vessels with a proliferation of the epithelium from the wall occurs. Rindfleisch* describes the development of tubercles of the omentum from the epithelium covering this; a method of origin at present somewhat problematical. The tubercle never exists primarily upon the free surface, but is always developed in the interstitial tissue of the organ; whether or not the development bears a definite relation to the vessels of the part, or to the lymph spaces, following a general law of development, is a matter as yet remaining undecided.

One finds in the cerebro-spinal axis the tubercles developing rapidly, within the course of two or three weeks, and terminating fatally in the form of a cerebro-spinal meningitis; or one finds the growth localized, generally in the gray matter of the brain, extending over a period of months, perhaps years, in the form of the solitary nodules made up of thousands of original tubercles.

This same distinction exists elsewhere in the body, most marked and most frequently seen in the kidneys, supra-renal capsules and testes. Whether a similar chronic tuberculosis of the lungs exists is a matter not to be asserted definitely. One often finds in these organs evidences of acute miliary tuberculosis combined with chronic cheesy conditions of inflammatory origin; in such cases, however, the former are secondary to the latter. And in fact it can be maintained

* Rindfleisch. Lehrbuch der Pathologischen Gewebelehre. Leipzig, 1867-9. P. 22?.

that acute miliary tuberculosis is generally secondary to preexisting disease of a chronic character, which at the same time may present itself in the form of the agglomerated tuberculous nodules and patches previously spoken of.

Tuberculosis of the lungs presenting perhaps the chief interest to the members of the Society, I will endeavor to bring together the main facts in the distinction between the cheesy conditions which form the most common cause of phthisis, and the tubercles of the lungs. As has been previously shown, the catarrhal secretion which is retained in mucous passages, especially the narrow ones, being made up in the main of cells, becomes cheesy through partial fatty metamorphosis of the cells and absorption of the liquid portions. With this fact well borne in mind, the explanation of so-called infiltrated tubercle of the lungs becomes easy. On opening the bronchi to their finer ramifications in these cases, the thickened, curd-like material is found lying in the lumen of the bronchus ; the lining membrane being swollen, thickened and rendered white by chronic inflammation, we have a clue to the origin of the round nodules with thickened capsule and softened contents, which are merely cross sections of the smaller bronchi. If the portion of the lung containing these masses is squeezed firmly between the fingers, the little plugs of altered secretion can be pressed out from the tubes, and present a similar appearance to the sebaceous secretion in comedones of the face.

As one recalls the structure of the lung, it will be remembered that the pulmonary alveoli sit upon the terminal bronchi, and that the lunima of the two are continuous ; the secretion being retained in the bronchi, it follows that the secretion is also retained in the alveolar recesses and undergoes a similar change. Hence the minute cheesy granules which are found in these cases. A chronic pneumonia is simply

one where this excessive secretion from the alveolar walls is retained and becomes cheesy. Neighboring portions of pulmonary tissue are in an œdematous condition, which œdema combined with a certain amount of cells in the alveolar spaces produces the so-called "gelatinous infiltration," and often one sees minute yellow specks in the midst of this gelatinous surface, which specks are nothing else than fatty-degenerated cells.

A thickening and condensation of the fibrous tissue about the bronchus produces a dense, firm mass which on length section manifests itself as gray, somewhat opaque lines; on cross-section one has a round border with a central cavity, the lumen of the bronchus. Does this development of fibrous tissue occur also in the intervals between the pulmonary lobules and about the vessels, one has a mass of cicatricial tissue which contracts, is firm, more or less colored with pulmonary pigment, and the appearance of so-called cirrhosis of the lungs is seen. If the chronic catarrhal condition with retention of secretion affects certain bronchi with their terminal ramifications at various parts of the lung, one has a relatively circumscribed broncho-pneumonia, which is an inflammation of the lungs proceeding from the bronchus. To such circumscribed forms of disease are due in great measure the so-called tubercles whether crude or healed, so often found at the apices of the lungs. A circumscribed inflammation of the pleura is set up, outside of these, hence the adhesions. As the nodule softens, becomes partially absorbed or cretified, depressions are produced by the contraction of the encapsuling cicatricial tissue, and the so-called tubercle is healed. In those cases where lobes of the lungs are converted into this homogeneous, dense, yellow mass, the cheesy pneumonia is of a lobar form; it may also be lobular if less territory is invaded. The true tubercle of the lung,

however, is always miliary, it never exists on the free respiratory surface, but always is imbedded in the tissue, whether in the bronchial mucous membrance, the interstitial fibrous septa, the sheaths of the vessels and bronchi, in the pleura, or in the adhesions between the pleural surfaces. If appearances supposed to be tubercles are found in the lung and not in the parts referred to, the probability, almost certainty, exists that such are due to modifications of the forms of chronic inflammation previously referred to, a probability rendered still more certain by the non-existence of tubercles in other organs of the body.

Niemeyer* asserts "that every form of pneumonia under certain conditions ends in cheesy infiltration, and in no form of pneumonia is the cheesy infiltration the constant and sole termination."

With regard to the first clause considerable doubt may be expressed whether the fibrinous form of pneumonia, the croupous of Rokitansky, ever terminates in this way.

The catarrhal cheesy pneumonia occurs especially in those persons called scrofulous, and in connection the bronchial glands become enlarged by the accumulation of lymphatic cells; these, too, become cheesy.

Though actual proof is wanting, it seems much more probable from the results of post-mortem examinations, that the fibrinous pneumonia terminates in resolution or death, and that the bronchial glands, swollen at first, undergo resolution in their turn if the case terminates favorably, the increased amount of cells disappearing.

Niemeyer states that in many cases the clinical history seems to prove his point, and that at times one sees a gradual transition from red and gray hepatization to cheesy infil-

* Niemeyer. Klinische Vorträge über die Lungenschwindsucht, von Dr. Ott. 2te Auflage. Berlin, 1867. P. 10.

tration. Does this change occur as a stage of one and the same process it must be exceedingly rare, and the objection always remains, did not the cheesy condition precede the attack of acute pneumonia.

Niemeyer also maintained that hæmoptysis* was in certain cases the immediate cause of phthisis, a view directly opposed to that of Laennec and Louis, who considered the same as a symptom of the disease already existing in the lungs.

The hæmoptysis produced the phthisis, in that the blood remaining in the bronchi and alveoli gave rise to a pneumonia the products of which became cheesy. This view was not a new one, but had already existed before the time of Laennec. This theory was supported in the main by clinical evidence. The results of experiments upon dogs and rabbits, however, furnish testimony opposed to this statement.

Perl and Lipmann† found that in bronchial hæmorrhage, artificially produced in the animals above mentioned, the blood did not enter the finest bronchi and alveoli, and that in the larger bronchi no coagula could be found after twelve hours. They also assert that the blood flowing into otherwise healthy air-passages does not act as a cause of inflammation, but is in part expectorated, in part gradually absorbed, the only alteration of the parenchyma of the lung being a moderate emphysema of the parts affected and the immediate vicinity.

Difficulties of diagnosis exist elsewhere, as in the testis, bones, &c., but a consideration of these points would demand more time and space than I am allowed. Then as to the intestines, we recognize a scrofulous ulcer arising from enlarged follicles and groups of follicles (Peyer's patches),

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* L. c., p. 48.

† Perl and Lipmann. Virchow's Archiv, vol. 51, 1870, p. 552.

and a tubercular ulcer arising from the presence of miliary tubercles. The two forms may be combined; an originally scrofulous ulcer may become tuberculous. In judging of these ulcerations, should we not find in the base and edges nor on the peritoneal surface the minute tubercles, the ulcer cannot be regarded as tuberculous. The presence of cheesy material does not suffice for the diagnosis of tubercle.

Tubercle occurs in nearly all the organs of the body either as acute or chronic; the former is more or less general, the latter, usually local at the outset, gives rise to a secondary infection of the vicinity or to a general infection of the various organs. The acute general form runs its course rapidly, as a rule proving fatal within three or four weeks, the symptoms resembling those of typhoid fever; the chronic form, so long as it remains localized, may exist for months and even years. Virchow has never seen tubercles in the salivary glands, pancreas and breast. The ovaries are rarely affected.

Primary tuberculosis of the muscular system, according to the same author, does not occur; a secondary form, however, is seen when the process advances from neighboring organs. Not unfrequently one finds miliary tubercles in the heart-substance, and Virchow has also seen here a large cheesy nodule which he considered to be of tuberculous origin. In 1867 Cohnheim* drew attention to the existence of tubercle in the choroid coat of the eye; at this time only four such cases had been recorded.

The clinical value of this discovery was placed in a strong light, and since then diagnoses of acute miliary tuberculosis have been made during life, by means of the ophthalmoscope.

Professor Buhl† of Munich, published the results of post-

* Cohnheim. Virchow's Archiv, vol. xxxix. p. 49.

† Buhl. Zeitschrift für Rationelle Medicin, 1857. Vol. viii. p. 50.

mortem investigations in 280 cases of tuberculosis and phthisis. In the vast majority of his cases of acute miliary tuberculosis, twenty-three in number, he found simultaneously cheesy nodules and cavities in the lungs.

He hence inferred that the acute miliary tuberculosis was an infectious disease produced by the entrance of the specific tubercular virus into the blood. Niemeyer* modified this view by stating that the tuberculosis in most cases was a secondary affection, produced in some unknown manner through the influence of cheesy diseased products upon the organism. Hence the presence of cheesy material of any sort might give rise to the development of tubercle.

Virchow opposed Buhl's theory from the fact that many cases occurred where cheesy material was present, scrofulous lymphatic glands for instance, and not only was there no resulting tuberculosis, but the same was absorbed and permanent recovery resulted. Then, again, there were cases where tubercles occurred in the absence of cheesy conditions, though at the same time he admits that such are extremely exceptional.

Within the past few years experiments have shown that Buhl's theory contains the germ of truth. It has been demonstrated again and again, that bodies having the appearance and structure of miliary tubercle may be produced in the various organs of the body. Villemin,† in December, 1865, published the results of his experiments, from which he deduced that the cause of tubercle is inoculable and can be communicated from man to rabbits. This fact, previously discovered by Klencke‡ in 1843, had been forgotten, and Villemin's results were such, that numerous similar experiments were made in various parts of Europe.

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* L. c., p. 26.

† Bulletin de l'Académie de Médecine. Waldenburg op. cit., p. 210.

‡ Waldenburg, op. cit., p. 198.

The inoculation was brought about in numerous ways; by direct injection into the bloodvessels, the peritoneal cavity, into the trachea, subcutaneously, &c. &c. Substances of the most varied nature were employed, not merely particles of the fresh, miliary tubercle, but also from cheesy tubercles, from cheesy pneumonia, pus fresh and crude, phthisical sputa, blood from phthisical patients, bits of malignant tumors, mercury, coal-dust, fat, &c. &c. Positive results being produced by the inoculation of dead and living, animal and vegetable growths, the doctrine of the specific tubercular virus as proposed by Buhl fell to the ground, and was replaced by the theory of Hoffman and Niemeyer; for it was found that in the majority of cases the direct result of the inoculation was the production of cheesy material in the part subjected to the experiment, then occurred secondarily the development of tubercle.

The results of experimentation are most carefully formularized by Cohnheim and Fränkel.* According to them, tubercle can be produced in guinea-pigs by the inoculation of gray, miliary granulations and from cheesy nodules. After it was ascertained that cheesy material produced this effect, charpie, gutta-percha and India-rubber were placed by them in the abdominal cavity.

Of the animals thus experimented upon, some died from general peritonitis, in others the inflammatory product became encapsuled, condensed. The inner surface of the capsule was in nearly all the cases studded with numerous small, gray, tuberculous nodules.

The general inference was, that "the dead and condensed pus received into the circulation served as origin for the tubercle."

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* Cohnheim und Fränkel. Virchow's Archiv, 1868, vol. xlv. p. 216.

Waldenburg* considers miliary tubercle to result from the absorption of finely divided corpuscular elements into the circulation, and their deposition, with the formation of nodules, at numerous scattered points of the various organs. Klebs† asserts that the tubercle virus is soluble in water, is precipitated on the addition of alcohol. On evaporating the aqueous solution the virus loses its efficacy. At the same time he finds it impossible to isolate completely the virus.

Clinical experience, then, as well as experiments upon the lower animals, show that in the vast majority of cases a connection exists between cheesy deposits and the presence of miliary tuberculosis. These cheesy deposits being in many cases the results of simple inflammation, a specific tubercular virus is not necessary for the production of tubercle.

Whether the irritation producing the growth of tubercle in any given part is brought about by the corpuscular elements introduced, or by the fluid surrounding and in them, the water of crystallization as it were, or whether an altogether new production arises from chemical changes in them, both elements and fluids, is unknown. The process of experimentation still goes on, the tendency is isolation, and science seems to have forced its way into the fissure at the bottom of which lies the immediate cause.

* Op. cit., p. 155.

† Klebs. Virchow's Archiv, 1870, vol. xlix. p. 291.

EXTERNAL MANIPULATION

IN

OBSTETRIC PRACTICE.

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READ JUNE 6, 1871.

EXTERNAL MANIPULATION IN OBSTETRIC PRACTICE.

The use of external manipulation in obstetric practice is a subject which has never received that attention from the medical profession to which it is justly entitled. Even at the present day the latest works on obstetrics make, as a rule, either no mention of this method of procedure at all, or else allude to it just as they would to any other purely theoretical but absolutely impractical suggestion, unworthy of being for a moment seriously considered, and still less deserving of being put into actual practice.

Even those writers who have recognized the possibility of the fœtal position being altered by external manipulation, have contented themselves with a passing allusion in their works to this possibility, without ever drawing from it any practical deductions, or considering whether some advantage might not be derived from its adoption into general practice over the older methods of performing version.

While the use of external manipulation in the *treatment* of obstetric cases has received but little attention, its application as an aid in the *diagnosis* of the fœtal position is a subject which has not been thought deserving of even a passing allusion.

Contenting ourselves with a hasty glance at the past history of what may be called modern midwifery, and omitting all mention of allusions to this subject contained in the writings of Hippocrates and others, whose works have more

a historical than a practical value, we find in one of the earliest works on midwifery,* which was ever published in the English language, that the midwife is instructed, in all cases of foot or arm presentations, to "cast the woman on her bed where she is to be directed to roul herself to and fro, or else she is to stroke the womb till she have reduced the infant to a better posture." In this advice, given as early as 1698 by John Pechey, is clearly recognized the possibility of the fœtal position being changed by external influences.

Whether this advice was ever followed, and if so, with what results, we are not informed. All we know is, that no mention of such a method of treatment appears in any of the obstetric works now extant, published from that time till 1807, when Dr. Wigand of Hamburg, published, in the Medical Journal of that city, an account of his experience in some cases in which he had tried external manipulation as a means of performing version. In 1812 he addressed to the Academies of Berlin and Paris a memoir,† in which he strongly advocated this method of treatment. The Hamburg Professor's views did not however become widely known beyond Germany till 1857, when his memoir was translated into French by Hergott of Strasburg. Still, even before the appearance of this translation, the paper awakened so much interest in the subject in Germany and France as to give rise to considerable discussion among the leading obstetricians of the day. On the one hand, Baudelocque, Capuron, Mde. Lachapelle and Gardien strongly dissented from the views of the German professor, while, on the other side of the question, were to be found Velpeau (1835), Lécorché-Colombe (1836), Cazeaux, Martin (1849),‡

* Compleat Midwife's Practice—Enlarged by Dr. John Pechey. 1698.

† Wigand : Von einer neuen und leichten Methode die Kinder zu wenden, in dessen drei, etc. Abhandlungen, Hamburg, 1812.

‡ Beit. zur Gynäkol. H. 2. Jena, 1849. Monatsschr. f. Gebrtsk. Bd. xvi. p. 1.

Nivert,* and especially Professor Stoltz, all of whom claimed that occasionally more or less advantage was to be gained by this mode of treatment.

So strongly impressed was Dr. Mattei,† a Corsican physician, with the improvement which such practice would bring about in the mortality of the lying-in-chamber, that he went so far as to advocate the examining of every woman even as early as the sixth month, with a view of altering any malpresentation which might be found to exist, apparently forgetful of the fact that, not unfrequently, the fœtus is found to alter its position spontaneously towards the close of pregnancy, and also unmindful of the difficulty of preventing a recurrence of the misplacement, even should the operation of version be successful at the time.

That Mattei had overestimated the value of the operation is clearly shown in a memoir, published in 1859 by Dr. Carl Esterle,‡ an Italian physician, in which, though favoring the application of external manipulation to obstetric practice, the writer strongly dissented from its being employed so early in pregnancy as Dr. Mattei had suggested. In support of his views, Dr. Esterle brought forward an account of 500 cases which he had examined, mostly in the seventh and eighth months, and among which he had discovered (the examinations being made of course by external manipulation alone) 22 complete transverse positions of the fœtus *in utero*. Of these, however, 9 rectified themselves before delivery took place. In 10 version was performed towards the close of pregnancy by external manipulation, in two by the so-called bi-manual method, and in one, owing to the peculiar complications of the case, resort to the old

* Nivert: De la version céphal. par man. externes. Gaz. des Hôpit., 1863, No. 47, 50, 51, 56. Monatsschr. f. Gebrtsk. Bd. xxii. p. 152.

† Gaz. de Paris, 1855, No. 23. Essai sur l'Accouchement Physiologique.

‡ Schmidt's Jahrbücher, 104.

method of version by internal manipulation was rendered necessary.

In Great Britain scarcely any attention was paid to this subject till 1860. It is true that Sir James Simpson had taught the great advantage to be derived, while performing version, from the application of one hand applied externally; but the advice had been given rather with the view of regulating the position of the uterus than of exercising any influence over the fœtal movements.

In the Lancets of July 14 and 21 of that year, Dr. J. Braxton Hicks published an account of a new way of performing the operation which he proposed to call "bi-manual version," but which was in reality merely a modification of Wigand's method. This operation met with the same cold reception with which the German professor's views had been received, and the old method of internal manipulation was still almost exclusively practised. November 4th, 1863, Dr. Hicks read an elaborate paper* before the London Obstetrical Society, in which he still further developed his views on the subject, and considerable discussion followed its reading in which many of the leading English obstetricians took part, among whom may be mentioned especially Greenhalgh and Barnes, who alluded to some cases in which they had adopted this treatment and with marked success.

In America, external manipulation has had, till within the last few years, no advocates at all. Prof. M. B. Wright, of Cincinnati, in his prize essay on "Difficult labors and their treatment," published in 1854, advocated a method of performing version very similar to that proposed six years later in England by Dr. Hicks.

Still but little attention has even as yet been paid to the subject in this country. Prof. Byford, of Chicago, gives,

* Transactions of the Obstet. Soc. of London, vol. v. p. 219.

it is true, in his work on Midwifery, an account of this mode of treatment, and recommends it in those cases in which the patient is seen early in labor; but, on the other hand, Prof. Bedford, in the last edition of his book on the "Principles and Practice of Obstetrics" (1870), still declares the whole subject of external manipulation in obstetric practice to be "*sub judice.*"

Such has been the previous history of a form of treatment which has, even up to the present time, found but little favor anywhere but in Germany. In Vienna the students are instructed to make out their diagnosis of the presentation by external manipulation alone, confirming their opinion, if necessary, by a vaginal examination, and are taught that, almost as an invariable rule, version can be performed by this method easier and with greater safety to the mother and child than by any other.

It is with the view of bringing this subject more prominently into notice that this paper has been prepared, in which I shall endeavor to state briefly the teachings and practice of the German School of Midwifery as regards the advantages to be derived from the use of external manipulation in the diagnosis and treatment of obstetric cases.

There are two ways of applying this form of treatment. The first is that revived by Wigand, which consists in using both hands externally; the second is the modification of this method, first described by Dr. Wright, but brought into notice chiefly by Dr. Hicks, in which, while one hand is employed externally, the other acts *per vaginam*.

As an aid to diagnosis, the former mode of procedure is alone of any very great value, while, as regards treatment, it will be found advantageous to employ sometimes one and sometimes the other of these two methods.

First, as an aid to diagnosis. External manipulation enables us to determine the position of the fœtus *in utero* long

before we could discover it by a vaginal examination. Mattei, who so strenuously advocated the early performance of version, was accustomed to examine patients with especial reference to the probable future presentation, even as early as the sixth month. Although any interference at so early a stage is clearly uncalled for and often useless, still the experience of the Corsican physician is valuable as showing the possibility of obtaining information on this point at even a very early period of pregnancy. By the ordinary method of examining, it is impossible to discover which variety of presentation we shall have to deal with until labor has actually begun, except in some few cases where the fœtus lies unusually low down in the pelvis and we are enabled to feel, even through the cervix, the round, hard tumor of the head or the softer and yielding tumor of the breech. It is only after labor has begun, the os dilating and the bag of membranes protruding, that a vaginal examination usually enables us to decide as to the variety of the presentation. Even after labor has fairly begun, where we have to deal with an excess of liquor amnii, or some forms of cross births (the very ones in which we should be most anxious for an early knowledge of the coming difficulties), or in cases where there is an unusually short or knotted cord, or where the cord is wound around the child, and in many other varieties of labor, where for any reason the fœtus does not descend low enough to allow of its being reached by the exploring finger, we are unable to decide until the labor has made considerable progress whether we shall probably be called upon to interfere in the course of the delivery or not. Nor is this all. A vaginal examination not only leaves us in the dark as to the coming difficulties, but is of itself a dangerous mode of attempting to make out the presentation in these doubtful cases, since we are liable to rupture the membranes and thus, should it afterwards turn out

that the fœtus occupies a transverse position, or that for any reason version is rendered necessary, an operation, which, before the *liquor amnii* escaped, was comparatively easy, is rendered more or less difficult for the operator and dangerous to both mother and child.

In cases where convulsions occur in the early stages of labor, we are enabled by external manipulation to recognize at once the fœtal position, and are thus prepared to interfere intelligently and terminate the labor with all possible despatch.

It is with a full recognition of the great advantages to be derived from an early knowledge of the position of the fœtus *in utero* that the students, who attend the courses on Operative Midwifery in the Hospitals at Vienna, Würzburg and Berlin, are taught most carefully to make out, from external manipulations alone, the probable presentation, the moment the woman enters the lying-in wards, no matter whether the labor has begun or not.

For this purpose the woman is placed upon her back, the knees drawn up, the operator standing at her side with his back towards her face. As a rule, at the beginning of the ninth month, the shape of the uterus is pyriform, the long axis lying in the direction of an imaginary line drawn from the ensiform cartilage to the symphysis pubis, the short axis being of course at right angles to this. It is owing to this peculiar shape of the uterus that the fœtus, whose outline *in utero* is also pyriform, usually presents either a vertex or a breech at the pelvic brim, the former of these presentations being by far the most common owing to the greater weight of the head, the fœtus floating in *liquor amnii* and consequently subject to the laws which govern floating bodies. Now when we come to make our examination, if we find that the short axis is from sternum to pubes, the probabilities are strongly in favor of its being a cross-birth. One very

marked exception is found to this general rule. In twins the pyriform condition of the uterus is frequently wanting. In these cases however we shall find, if we examine the uterus externally by palpation, a well-marked depression running across the centre of the tumor. Should the eye detect nothing abnormal in the shape of the abdomen, one hand is to be used to steady the uterus, exercising a gentle pressure over one iliac region, while the other firmly presses over the corresponding region of the other side.

If it is a vertex presentation, a hard, round tumor will be felt either in the right or left iliac region, usually of course in the latter, while the softer tumor, formed by the breech, can easily be detected at the *fundus uteri* on the same side of the median line as the vertex occupied above the pelvic brim. In breech presentations of course this condition is reversed, and very little practice will enable us to decide at once whether the tumor above the pubes is formed by the yielding breech or resisting head. In face presentations the tumor above the brim, not being formed by the vertex but rather by the parietal bone, is found more directly over the symphysis pubis, thus occupying a central rather than a lateral position. The breech usually occupies the same position as described above in speaking of vertex presentations.

In transverse positions of the fœtus all these conditions are changed. No tumor can be detected above the brim nor at the fundus. Just below the umbilicus will be found, on either side, the two tumors formed by the head and breech, the latter occupying, in by far the larger number of cases, a little higher position than the former; a fact which, if remembered, will frequently enable the practitioner to decide, irrespective of the relative degree of resistance imparted to the finger by either tumor, where the head is and where the breech. The fact, also, that, when the child is lying

with his back to the spine of the mother, the tumor, formed by the flexed thighs, will present an unevenness not detected when the hand is examining the opposite condition of things, gives us an easy guide for detecting with great accuracy just how the child is lying, and which arm or shoulder is over the as yet undilated os.

Without going over all the different varieties of transverse presentations and giving the distinctive features of each, as made out by external manipulation, I have, I trust, sufficiently illustrated this method of making an examination. To one who has never had any experience with this mode of examining a patient, much that is here written will seem to be easier in theory than in practice. Any one, however, who will watch the attending physicians in the German lying-in hospitals, and will notice with what apparent ease and almost absolute accuracy they make out their diagnosis of the fœtal positions from external manipulations alone, will soon be convinced that much valuable information can be acquired by an investigation made long before a vaginal examination would have yielded the slightest results.

In certain cases of fat subjects, where there is a large deposit of adipose tissue, this method of procedure is, as a rule, unsatisfactory; but even then, some facts of importance may frequently be discovered.

In all cases, before making the examination, care should be taken to see that the bladder is empty. If necessary, especially in nervous or hysterical subjects where for any reason we are led to suspect anything abnormal, an anæsthetic may be given and the examination thus greatly facilitated.

2. As an aid in the treatment of obstetric cases. It is chiefly in this connection that external manipulation is of especial value and deserving of far greater consideration than it has yet received. By this method of treatment the operation of version is rendered one of comparatively little dan-

ger either to the mother or child. I am aware that version is rarely called for, but the question is still debatable as to whether the mortality of the lying-in chamber would not be considerably lessened were it more frequently performed.

According to the best statistics* the operation occurs in England about once in every 269 cases of delivery; in Germany, however, it is met with as frequently as once in 63 2-3 cases. According to M. Riecke,† who has carefully collected the particulars of 3120 cases in which version was performed, the operation resulted fatally to the mother in 600 of them, while of the children thus delivered 1756 were lost; or, in other words, the result was unfavorable to the mother in one out of every 5.2 cases and to the child in one out of 1.77. By far the larger number of fatal results to the mother arise either from rupture of the uterus at the time of the operation, or subsequently from some form of uterine inflammation. Both of these accidents are principally if not entirely due to the necessity which the mode of performing the operation calls for, of the introduction of a hand, and not unfrequently a portion of the arm, within the uterine cavity. The great mortality among the children thus delivered undoubtedly arises from the difficulty frequently experienced in accomplishing the rapid delivery of the head, after the rest of the body has been expelled, a delay which too often causes the child to die of asphyxia occasioned by the prolonged pressure to which the cord is subjected.

The danger to the mother of rupture of the uterus or the subsequent inflammation of that organ, is at once greatly diminished if the operator, discarding the old method which required for its performance that the whole hand should be introduced within the uterine cavity, will use the "bi-man-

* Theoretical and Practical Midwifery. Cazeaux, 1866, p. 789.

† Riecke, L. v. Geburtsh. Operationscursus. Tübingen, 1846.

ual" treatment as described in detail by Dr. Hicks, a form of practice which requires at the most only the introduction of two or three fingers within the cervix uteri, or, what is better still, external manipulation, as recommended by Prof. Wigand.

On the other hand the prolonged pressure exerted by the head on the cord, to which I have alluded as being the chief source of danger to the child, can be avoided by the operator's performing cephalic rather than pelvic or podalic version; a form of the operation, however, which it is exceedingly difficult to accomplish by the old method of internal manipulation.

To illustrate external version, let us suppose that we have to deal with the most frequent shoulder presentation—namely, the right anterior dorsal—the labor not yet having begun. The patient should be placed on her back, her hips elevated and resting on a pillow, the knees drawn up. The bladder and rectum should be empty. The operator stands either on the woman's right or directly in front facing her. An examination, made by external manipulation, discovers the round, resisting tumor formed by the head in the left iliac region, and the yielding irregular tumor of the breech occupying a corresponding position in the opposite side. The right hand (if the operator stands facing the patient, otherwise the left) grasps the vertex as felt through the abdominal walls; the left hand encircles the breech.

The operator is now ready to turn, and he may either perform cephalic or pelvic version. It is desirable, if possible, to perform cephalic version so as to avoid any subsequent prolonged pressure on the cord; but occasionally it will be found preferable to bring down whichever extremity lies nearest to the pelvic brim, so as to render necessary as little movement of the fœtus as possible. If the operator decides

on performing cephalic version, the hand which grasps the head is to make firm pressure downwards and inwards, while the other presses upwards and inwards, both hands thus moving in the same circle. The operation having been accomplished, the patient should be kept lying on the side corresponding to the position originally occupied by the head; a pillow being placed under the head so as to prevent its returning to its original condition. In some cases, especially where there is a marked deviation from the normal form of the uterus, a bandage properly applied will help to correct those uterine obliquities to which, as Kilian* has suggested, the malposition of the fœtus is occasionally due. Where a bandage is thus applied, the decubitus of the patient, after its application, should be dorsal.

If labor has begun, the operation is the same, except that pressure is to be exerted only in the intervals between the pains. During a pain the patient should be placed on the side corresponding to the position of the head, and gentle pressure kept up on the head either by the operator's hand or by means of a pillow placed beneath it. The pain having passed off, the dorsal position is to be resumed and the operator proceeds as before. As soon as the head is in the right position the membranes may be ruptured, and the subsequent contraction of the uterus will usually suffice to prevent any recurrence of the displacement.

In those cases where the *liquor amnii* has already escaped, the uterus is, as a rule, contracted more or less firmly about the child, and the pains are found occurring frequently and with considerable severity. In these cases it would seem at first hopeless to attempt to perform the operation except by the old method. It is however in these very cases that

* Kilian: Die operat. Geburtshülfe, Bonn, 1849.

version by internal manipulation is attended with the greatest difficulty and with the most danger to the mother, as it is under just such conditions that rupture of the uterus is most apt to occur. In these cases the administration of ether will frequently cause the severity of the pains temporarily to abate, and, the contractions of the uterus for the moment relaxing, the operator will very frequently be able to turn by external manipulation. Should he however be unsuccessful, bi-manual version should be attempted. This operation is founded chiefly on the fact that, in transverse presentations, the knee lies, as a rule, directly over the os and consequently within a finger's length of it; while the foot lies next to the breech, thus requiring the introduction of one or more fingers only a short distance within the os to reach the knees in those cases where we wish to perform podalic version. In by far the larger number of cases however the object of the physician should be to accomplish the cephalic form of the operation. Here the hand applied externally acts upon the vertex and endeavors to depress it towards the pelvic brim; while the fingers, passed through the os and applied to the presenting shoulder, are to be used to press that part of the fœtus on in the direction of the feet. When the head has reached the fingers, which are waiting to receive it, very little effort is needed to adjust it in whatever position may be desired. The child should be detained in its new situation until it is evident that the head is fairly engaged in the brim. If necessary the membranes may be ruptured so as to hasten the contractions of the uterus. Care should always be taken, in performing either bi-manual or external version, to avoid a face presentation.

Should neither of these two methods of operating be successful, we are still able to resort to the old method of internal version. The bi-manual operation is difficult of

accomplishment in cases where the child is dead or premature, as there is then a lack of rigidity and elasticity on the part of the fœtus, which greatly impedes the rapid performance of the version, the fœtus frequently doubling up on itself.

In the lectures on midwifery delivered by Prof. Carl Braun, in the Imperial Hospital at Vienna (1869–70), the students were instructed to alter, as early as possible, by external manipulation alone, all malpresentations. Particular stress was laid on the statement that labor should never be allowed to begin in such cases without the fœtal position having first been rectified. Mayrhofer, Prof. Braun's first assistant, and Späth, to whom was intrusted the instruction of the midwives in the Vienna Hospital, taught the same doctrine, as did also Scanzoni at Würzburg and Martin at Berlin. Even after labor has begun, external manipulation is always the method first tried. In the hands of the German operators this form of treatment is eminently successful. In one case, in which the operation was performed before a class of midwives, Späth brought to the pelvic brim, in order to show the facility with which the version could be accomplished, first the head, then the breech, and finally the head again, which was then allowed to remain, and the labor soon terminated favorably.

In cases of placenta prævia, of whose existence notice has been given us by one or two sharp dashes of hæmorrhage, we have in bi-manual version a method of operating which enables us to interfere long before we could introduce our hand within the dilating os. The moment one or two fingers only can be introduced, version can be performed, and, the feet having been brought down, we have in the body of the child a tampon which will serve our purpose better than any other.

The same method of procedure holds good in cases of

accidental hæmorrage and in convulsions occurring before labor has fairly set in.

In prolapse of the cord, Dr. Wright's method enables us, as soon as the os has dilated so as to admit of the introduction of only one or two fingers, to deliver the child rapidly by podalic version, thus avoiding all long continued pressure on the cord, which must necessarily follow if we are obliged to wait until we can turn by means of the hand introduced within the uterus.

To sum up briefly the advantages to be derived from the application of external manipulation to obstetric practice, we find—

1st. That the diagnosis of the fœtal position can be made out before labor begins, and while the membranes are unruptured.

2nd. That the examination thus conducted can be made with less discomfort to the patient and less trouble to the physician than attends a vaginal examination.

That, as regards the treatment of obstetric cases, there are many very decided advantages to be derived from the use of external manipulation—

1st. That version can be performed before labor begins, thus enabling us to convert, with the greatest ease to ourselves and safety to our patient, an unnatural into a natural presentation.

2nd. That version can also be performed during labor with much less danger to both mother and child than is possible by any other method.

3rd. That by this treatment cephalic version can be performed as easily as either podalic or pelvic.

4th. That in cases of placenta prævia we can interfere at a very early stage of the labor, and adopt a form of treatment which offers far better chances to both mother and child than any other.

5th. In cases of prolapsed funis, by adopting Wright's method, we can terminate a case of labor much earlier than if we were obliged to wait till the os should dilate sufficiently for us to introduce our hand.

6th, and lastly. In cases of accidental hæmorrhage or convulsions, we are enabled to interfere earlier and terminate the labor more rapidly than has hitherto been thought possible.

In presenting these views, I am aware that they will seem to many fanciful and theoretical. They require only a trial to be found to possess great practical value; for there is no reason, clear to my mind, why the American obstetricians should not be able to acquire a dexterity and skill in the diagnosis and treatment of obstetric cases by external manipulation as great as is daily shown to those students who attend the midwifery clinics in the large Lying-in Hospitals of Germany.

AUTHORITIES NOT DIRECTLY REFERRED TO.

Allgem. Wien. Med. Ztg., Dec. 1863, No. 58.

C. Hecker :—Klin. d. Gebrtsk, Bd. ii. p. 141.

Monatsschr. f. Gebrtsk. Bd. xxvii. p. 219.

Hohl :—Lehrb. d. Gebrtsk. 2 Aufl. Leipsic, 1862, p. 784.

Schmidt's Jahrbüch, 1857, Bd. xcv. p. 66.

Busch :—Adhandlungen, etc., 1826, p. 27.

d'Outrepont, J. Von der Selbstwendung u. d. Wend. auf den Kopf. Ein Progr. Würzburg., 1817. 8.

VENESECTION,

ITS ABUSE FORMERLY—ITS NEGLECT AT THE PRESENT DAY.

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JUNE 6, 1871.

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VENESECTION.

Gentlemen :

I have long felt that some one ought to speak upon the above subject. No one has done so in Massachusetts. Having had a little but a very *keen* experience, authorizing me, as I think, to speak, I am here for that purpose. I wish to draw the attention of the middle-aged practitioner who has lost his own lancet or who keeps his father's in a rusty state in his pocket or in some corner of his study, as a mere memorial of the past. But more especially do I desire to interest the young men just entering the profession, who have been taught by wise instructors that bleeding is a process not to be resorted to, at present, in hardly any case, because, forsooth, 1st, one cannot hope to strangle an acute disease; and 2d, because it is better *never* to bleed than to do so extravagantly, as our fathers did, for they sometimes killed their patients by too much bleeding, &c. &c. But we, if we follow such doctrines, will, as I believe, let some of ours die from neglecting the remedy.

Amid the great changes that have taken place in medical opinion during the last half century, I know of no remedy that has suffered more in the estimation of physicians and of the public than this of Venesection as a Therapeutic Measure.

I have seen it, at one time, used absurdly and without stint. I see it now almost, if not quite, abandoned by all. Students are not taught the art as among the legitimate objects of minor surgery; and old men shrink from performing it because so long unaccustomed to it.

Early in my practice I frequently refused the solicitations of healthy men and women, asking for venesection, they having been bled annually for a long time, and, as they stated, with great comfort to themselves. I have known Bouillaud and his followers, in their zeal for venesection, "*coup sur coup*," go far beyond the bounds of reason and of prudence.

I admit that it is quite natural, and also exactly right, that the strongest anathemas should be hurled by professional and lay opponents against such extravagances in blood-letting. But while admitting most freely all this, I contend that we, of the present hour, have fallen into the opposite extreme of folly, and that the opinions held now by the profession, in consequence of which blood-letting is so wholly neglected, are as irrational, though perhaps not so widely injurious, as the Sangrado mode of practice of former days. The bleeders have now been so effectually silenced, that we have virtually thrown aside, as worse than worthless, one of the most valuable of all the therapeutic means which the long experience of the ages has taught us.

It is in the hope of doing something towards bringing the profession back to reason on this point that I agreed to prepare this paper, based upon my limited experience in venesection. I shall cite four cases as illustrative of that experience. They are spread over forty years of my professional life. Their records and my running commentaries upon them will, I think, tend to show—*In the first case*, how human life was probably shortened and certainly made more miserable by venesection, as carried on by our fathers

in medicine. *In the second case*, we shall find all the severer symptoms of a violent, acute, cardiac disease were instantly relieved by venesection, and the patient brought from a state of great torture to one of comparative comfort, and although the disease was not "strangled," it was apparently made milder, and ran favorably its course up to perfect health. *In the third case*, all the severer symptoms of congestion of the lungs, combined with great obstruction of the heart and dropsical effusions in various parts of the body, occurring in a case of chronic hypertrophy of the heart with valvular disease, were manifestly and decidedly relieved after, and, as I believe, in consequence of venesection, the patient gaining thereby a "new lease of life." And finally, *in the fourth case*, I know that coincident with a neglect of venesection came death, and my belief is that possibly, nay probably, if venesection had been performed, life would have been saved.

The four cases illustrate the three epochs of medical opinion on the question of venesection, and foreshadow, I trust, the fourth that is about commencing.

First—The epoch of arrant folly that sanctioned and required an extravagant and wholly improper use of venesection.

Second—The period of scepticism as to its value in many cases, and a complete disapproval of the inordinate use of it as in the previous epoch.

Third—The effete epoch of actual cowardice in its use, and virtual abandonment of the remedy which is demanded by many at the present time.

Fourth—The *hoped-for period* of the future, when venesection, like other powerful remedies, *will be used rationally* and to the saving of human life.

Case I.

Forty years ago, when House Physician of the Massachusetts General Hospital, I protested against venesection at the very moment that by orders of my superior, the attending physician, I opened a vein in the arm of a girl. The following are the circumstances of her case: She was 21 years old, unmarried. Her mother had died with disease of the heart. The girl entered the Hospital June 21, 1831. Three years previously she had been there with what was called "neuralgia," and she then had pain in the left side of the chest, with dyspnœa. She was also unable to raise her left arm. These symptoms had followed after a fall from a chaise, resulting in a fracture of the clavicle, and some injury to the ribs. For two years and a half previous to her entrance, when I first saw her, she had had violent attacks referable to the heart. She stated that the first attack was thought to be one of croup, and in all subsequent attacks she had had frequent desire to raise phlegm from the throat. They were however chiefly marked by very great palpitation and dyspnœa, with a choking sensation and a very rapid and feeble pulse. They had occurred quite often; and for them she had been bled 93 times—i. e., once in 10 days during two years and a half! The catamenia had been regular and natural. She had had some dysuria. She entered at the beginning of a paroxysm, and was suffering so much that no full account could be obtained of her history. I reported her as being in bed, looking very thin and pallid. The superficial veins were every where distended with very pale-colored blood. She was panting, and could not easily speak. Her heart throbbed so violently that the bed-clothes were moved by its jerkings. The carotids were also beating strongly. On auscultation, the motions of the

heart were very tumultuous, but no evident valvular or any other morbid sound was found. The pulse was 160. I regarded the attack as plainly of a nervous character. She wanted me then to open a vein. I declined, on the ground that probably a part of the urgency of her symptoms was induced by the too frequent bleeding by some foolish predecessors. The attending physician of the hospital saw her the next day and prescribed —

Inf. valerian., ℥ij.;
Acet. opii, gr. xxx.;
Spt. ether. sulph., gtt. xl.;

to be repeated in an hour. Fomentations over the heart.

The prescription did no good, and in the evening the dyspnœa increased. At times the breath seemed to cease, the face becoming livid. The eyes were closed, and she uttered slight groans. Meanwhile the heart and carotids were intensely throbbing. Pulse 140.

℥i. mistur camph. and 60 drops laudanum every two hours.

Under this she vomited in about an hour, with relief, though the paroxysms at times returned, and the pulse fell to 120. On 22d more easy, but still some dyspnœa. The tongue was dry, the lips and gums of a bloodless hue. She was very weak. Assafœtid enemata were ordered, also vin. colchici and digitalis with tinct. opii. Unfortunately, as I thought at the time, and think so now, my superior ordered me *in her presence* to bleed her, *if she thought it necessary*, and if a serious attack should again occur. Accordingly that afternoon, the remedies having been unsuccessful, she sent for me and said that the time had come, and that, according to orders received, I must bleed her. Unwillingly I opened a vein (the 94th time!). The blood flowed so freely, and was of such a pale color, that at first I feared I had acci-

dentally opened the artery. I drew eight ounces of blood, and as she had noticed at previous venesections, the stopping of the vein was very difficult. Only partial relief of the symptoms followed.

She remained in the hospital until July 7, i. e., a little more than a fortnight, and was discharged as insane. During that time she had the following symptoms: She often felt faint and had neuralgic pains in the face, neck and legs, with at times stiffness in the neck; choking sensations and vomiting occurred not unfrequently. The bowels were regular, except when acted upon violently by medicine. She had at one time some frothy sputa. She had headache and poor sleep, and finally delirium came on, at times violent with fantastic movements, dancing, &c.; at other times she would lie with her head under a pillow and call on the attendants to lay her in her coffin. She left July 7, and died about two weeks afterwards.

No disease of the heart was found; it was only slightly enlarged. The ovaries were a little diseased; the liver was normal, but larger than usual.

This, in my opinion, is one of the most infamous cases on record—one disgraceful to the profession which could be so misguided as to suppose that any such "heroic practice" could ever end save in discomfiture. Though at that time a tyro in medicine, my reason revolted then at the whole course that had been pursued, and it revolts now as I think of it, and I presume you will all agree with me.

Venesection repeated as in this case, justly caused indignant protests on the part of the profession and of the public. But is it not true, that swinging, as human reason is so apt to do, from one extreme of opinion to another, we now, after a lapse of forty years, have gone to the opposite degree, if not the same kind, of folly? We now wholly

neglect, as we formerly misused, one of the most valuable of remedies.

In order to illustrate this latter point, permit me briefly to allude to three more cases.

Case II.

December 6, 1853, i. e., eighteen years ago, and more than twenty-two years after the case just reported, B. M., a married woman, aged thirty, entered the Massachusetts General Hospital, under my charge as attending physician. Having been previously in good health, she had been seized a week before her entrance with a severe attack of acute rheumatism of the extremities. She had also had pains in the cardiac region, and some dyspnœa from the first. The House Physician reported her as groaning with pain in the cardiac region, to which leeches and a blister had been applied. She had pains also in her knees, hips, wrists and joints of her fingers, increased by motion, but scarcely any redness or swelling of them. The pulse was 100. Her skin was hot and dry, except that a slight sweat was on the face. The tongue was dry, with a thick, yellowish white coat. The House Physician ordered :—Opii i., ipecac 1-4, every four hours.

Next morning I found her suffering agonizing orthopnœa. The respirations were seventy-two per minute, short and cut off by pain in the cardiac region. Six leeches had been applied over the heart and were then bleeding freely. No dejection for two days. Pulse 108, very small and uneven, evidently laboring. The sounds of the heart were distant and the impulse was scarcely perceptible. No endocardial or pericardial morbid sounds could be heard. The percussion,

though imperfectly practised owing to the suffering condition of patient, was not remarkable, but it gave, perhaps, a little larger dulness than usual. The respiratory murmur was heard to the third rib of the left breast, but not below. The abdomen was quite tympanitic, the sound of it extending up as high as the nipple. Some bronchial respirations without râles in the lower parts of both backs, and on percussion there was rather less sound than normal in the same parts.

I stood by the side of the suffering woman; I watched her agonized expression, felt of her weak and uneven pulse and counted her catching, panting breath, cut off by the acute pain. There was evidently a rheumatic inflammation of a portion of the heart, whereby the organ seemed to be much obstructed in its movements. It was plain that something must be done speedily for her relief. The leeches and internal remedies seemed of no avail. I rapidly reviewed all, and argued as follows. This woman has a stout, able-bodied frame. She has been ill only one week. The rheumatism has attacked the heart. It has already influenced it. I cannot tell the exact condition of the organ, but it is plain from the pulse and from auscultation that it cannot move easily. Here has arisen obstruction to the circulation of the blood. Vain endeavors, on the part of nature, are now making by rapid breathing and the rapid, feeble pulse to overcome the difficulty. Is not the heart now, in fact, crammed full of blood? Can the loss of over a pint from this full-blooded, well-developed frame do any harm? Is not the organ, indeed, at this moment calling upon me by these signs to give it relief by removing a certain amount of what it is almost vainly struggling with? Can there be any harm in trying this remedy, while watching closely its effects? Would the old fathers in medicine have stood doubting as I am now? Nay, would they not have bled

her at the first moment that this condition of things commenced? Should modern professional investigations make me *discard* bleeding. I remember, moreover, that I have not bled for many years. Perhaps my lancet, if I have one in my pocket, is dull. Every one in this ward where I am now standing in doubt as to what is my duty, will be surprised if bleeding be performed, and that, too, while the leech-bites applied by my assistant are still open.

These, gentlemen, and other similar arguments flashed with lightning rapidity through my mind as I still held the pulse of my suffering patient. There was, however, really but one moment of real doubt, and I then felt sure that it was not only feasible, but that it was *my duty to my patient* to open a vein and to watch the effect. I hastily bound up the arm and bled her to sixteen ounces. The anguish of expression instantly subsided, and never returned afterwards. She could draw a full breath, and when I finished the bleeding, the pain in the side and all the *violent* symptoms had disappeared as if by magic. She leaned back upon her pillow, a little exhausted, but relieved of her previous torture. I ordered a cathartic of jalap and calomel of ten grains each, and according to the usual practice of that day I gave a compound calomel pill at night.

Sinapisms were also applied to the knees and ankles. The blood had a very firm coagulum. It was "buffed and cupped." The patient staid in the house three and one-half months, and went out "well."

It is not necessary for my object to give the history of all the subsequent phases of the disease. Suffice it to say that she never again had the same persistent dyspnœa. The venesection cut off that and also the severe cardiac pain. She did, however, occasionally have attacks of slighter dyspnœa lasting for ten or fifteen minutes at a time, with

some distress in the cardiac region. The rheumatism showed itself more distinctly, with redness and swelling of the joints. The physical signs about the heart were always somewhat obscure. A little prolongation of the first sound was at one time noticed, and a doubtful precardiac rub was heard, and there was more dulness than usual in the cardiac region. There was also evidently obstruction of both of the lower lobes of the lungs, marked by bronchial respiration, a change of vocal resonance, and subsequently by râles. There was a little cough. The pulse during the venesection became fuller, and it fell in rapidity. The respiration, though relieved of its severity, remained for some time somewhat shorter and quicker than normal, but it was noted at 32, four days after the bleeding.

She had mercurials to a slight touching of the gums, and subsequently colchicum, opiates and blistering over the heart and lungs.

The great and immediate advantages of venesection in this case were manifest, and although it did not prevent certain processes from going on in the heart and lungs, it did cut off *instantly the severity of their influence over the patient*. It seemed to give to nature that aid which she needed for the processes she was at the time vainly struggling to carry on, with great agony to the patient.

The motto of our Society is, "Natura Duce." Let us remember always that while taking Nature as our guide, we must nevertheless use our own reason and not allow Nature to run riot in the way. We must in fact govern the powers of nature to our own good. The above motto, unless accepted with these limitations, has a most disastrous effect. She indicates that the loss of blood may destroy life. Blood is the pabulum whence all our energies flow. Hence we have jumped to the conclusion, that the loss of *any* of it is

always a serious *evil*, more or less prejudicial to the patient. Hence, too, has arisen on the part of opponents of venesection this protest against it under *all* circumstances. Our lancets grow dull and our young men are not taught even the process of venesection. And why?—simply because physicians formerly undertook to storm nature, as it were, by venesections, "*coup sur coup*," as Bouillaud forcibly expresses it, and now we have run far to the opposite extreme, and instrument-makers have given up the keeping of lancets, for no one bleeds.* There never was a greater folly than this almost universal protest against venesection under *all* circumstances that may arise. And this is made upon the totally false assumption that the loss of any blood is always injurious. Look for a moment at the numerous exceptions which nature herself offers us. Every month, one-half of the human beings who may be alive prove, by an exorable law of their natures, the falsity of the assertion. Every school-boy, who at puberty has his nose bleed, proves its folly. Every headache cured by epistaxis confirms the truth of my assertion. Every child born into this world forces its mother to submit to this benign law of venous depletion. Not a week passes that some surgeon does not see his patient leap up to life after a bloody operation. Why, then, all this timidity in regard to the opening a vein in the arm in cases of extreme suffering, where, for instance, there are some severe symptoms threatening death or causing extreme distress, either in acute disease, or even in chronic disease when acute symptoms are superadded and threaten life? To illustrate the latter part of the proposition, let me now present the next case.

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* See Address by Dr. B. Fordyce Barker, alluded to later in this paper.

Case III.

This case fell under my notice exactly forty years after that first related. In its record of suffering and of prompt relief from venesection, it contains an answer to almost every argument that the opposer of venesection may bring up.

Nov. 16, 1870. C. H. M., aged 36, engineer and government contractor for dredging the harbor in Boston. He was a very stout, stalwart young man. He had been accustomed to violent straining while engaged in his laborious work. His father died from intemperance at 46. His mother lived to 74, but had cough every winter. The patient had usually enjoyed good health, except that occasionally he had had rheumatism. May, 1870, he felt, while in bed at night, his throat suddenly fill with much frothy matter. It came so freely that he could not speak. He thought at first that blood was escaping, the lungs filled so rapidly. From that time he had had a cough, worse at night, with wheezing. But he had been able to work till within a few weeks. He had been for months under the treatment of quacks for "worms." My assistant, Dr. Knight, saw him late in the summer, during my absence in Europe. The patient then had much dyspnœa, and had had little sleep in consequence thereof. He could not lie down. He was, however, able to visit my office in Boston, but he suffered much in so doing. While there his respiration, according to Dr. K.'s record, was hurried and rattling. He still raised much frothy adhesive matter, more than half a pint in twenty-four hours. He had never had hæmoptysis. Some sweating at night and chills afterwards. Appetite poor; no distress from food; bowels open by injection; urine high-colored, about one pint daily. Pulse 110, full and quick. There was an

increased area of cardiac dulness. The apex was lowered and carried outward. The heart's sounds were heavy and muffled, with a loud systolic and slight diastolic murmur over the aorta. Lungs full of coarse râles, dry and moist. Tinct. iodin. over heart. Lin. camphor over chest.

℞. Pulv. digitalis,
" scillæ,
" colchic. semin., āā gr. i. 3 t. d.

19th. Worse. Cough troublesome.

Pulv. digitalis, gr. v.;
" belladonna, gr. i.;
Ferri. redacti, ℈ ij.;
M. Ft. pil. No. xx.
Emplast. belladonna, 4×4, over heart.

I saw him at his own house about the middle of November. He was then unable to leave his chamber, owing to orthopnœa and great suffering. He had no pain about the heart, though occasionally he had stitches through the shoulders and breast. He had had for a long time some tendency to swelling of the feet. The urine at times had been heavily laden with deposits. The cough had often been very severe, causing lividity of the face. He had been able to lie down a little the previous two nights.

At my visit he was sitting up, apparently not in very great distress. The pulse was 76 and regular, but evidently confined and small. Tongue clean. Respiration 28, regular, with a little wheeze. No œdema of the legs. Impulse of heart and flatness on percussion over large space and extending towards the left. Sound of aortic regurgitation heard down almost to the bottom of the sternum. Behind, nothing morbid excepting some coarse râles less extensive than when examined by Dr. Knight.

Last pills twice daily.
Take digitalis, gr. $\frac{1}{4}$; colchicum seeds, gr. i.; and soda bi-carbon. $1\frac{1}{2}$ gr. at night.
Iodin. ʒss.; ether sulph. ℥i. over heart.

Ordered an open wood fire for room. If cough became severe, he was to take—

ʒi. p. r. n. of
℞. Syrup tolutan., ℥ii.;
Vin. antimon., gr. xx.;
Fluid ext. opii, ʒi.;
Spts. ether. nitros., ʒii.

Nov. 23. Doing well till this morning, when he had a severe attack of dyspnœa, during which he almost lost his breath, and was thought dying. At my visit his pulse was full and regular; no wheezing. Full effect of iodine. Inhale vinegar and water; and if a severe attack, inhale sulphuric ether.

24th. Terrible dyspnœa. No dejection.

Jalap., gr. xii.; cream of tartar, ʒi.

27th. Accesses of dyspnœa increased in severity. He had great œdema of legs and face. His nights were full of terrible suffering. The lungs seemed more obstructed. It was evident that in one of these accesses of orthopnœa he would die unless relief were obtained. As he was stout and full of blood, I urged Dr. Wheeler to bleed him in case he should have another severe attack. We had much discussion about the propriety of doing this, but all argument was effectually silenced by our united thought that though we could not hope to cure the chronic disease, there was evidence that the heart had become seriously obstructed. The final question was — shall we stand by and see him die from an attack of orthopnœa, when perhaps venesection, while it could do no harm to such a young man, would almost cer-

tainly give temporary relief, and might really help him to get over this acute attack? Our conclusion was—it cannot do permanent harm; it may do good. Hence by every principle of our art we were bound to give our patient the chance of even partial relief. An attack came on that night, and Dr. Wheeler bled him about thirteen ounces, with the greatest relief to all the symptoms. He had some pain low in the right back for a day or two afterwards, for which Dr. Wheeler cupped him to the amount of a few ounces, with still further relief. There was some rude and an approach to bronchial respiration, and some dulness on percussion in same part. But the patient *never had a violent access of dyspnœa* after the venesection. And he and the attending physicians and friends all admitted that the first step towards relief came with that operation. Soon the physical signs lessened, with improved rational symptoms. Ordered quinine gr. $\frac{1}{3}$ every three hours. Allowed to have ice cream and broths. The improvement continued in a manner most surprising to both of us. Dec. 12th, i. e. the fifteenth day after venesection, he was up and dressed, and wanted to know when he could go out of doors. He looked like a different person. He had scarcely any dyspnœa. He could lie down quietly at night. The cough and sputa were much less. He had no pains anywhere. Said he felt better than for some months past. The dropsy of the legs and face had wholly gone. Though some dulness existed in the lower parts of the right back, the râles were much less. The heart's action was more free, though the murmur was still heard and the hypertrophy was still manifest. Pulse 92, regular. He ate well and slept without an opiate. Finding himself as he thought so entirely well, he became very imprudent in spite of our persuasions to the contrary. On the 13th December, i. e. the 16th day from the venesection,

he went to Boston and walked up to one of the highest offices in State street, and there signed a contract with the United States for another four years of dredging. On the 17th, i. e. only 20 days after the venesection, I met him in the train from Portland, where he had gone "as a joke" to surprise his relations "who thought him dying." He looked a little pale, but otherwise as well as any one in the train. He walked not as an invalid, and scorned any one's assistance. Knowing his organic disease, I cautioned him very earnestly, but with little effect. He laughed at my fears, but he promised caution, which, however, I foresaw he would not take. As I left him that evening, I could not help thinking what a strange commentary his case afforded on the *dangers* of blood-letting, about which so much is heard of late.

Continuing to act with the same recklessness, exposing himself on the sea shore in cold and wet weather, &c., he fell ill again. The dyspnœa returned. January 20th, about a month after venesection, I saw him again. He was in bed, evidently very low. He had sat up three nights expectorating bloody mucus. The heart sounds and impulse were obscure, as was the respiration also. Paroxysms of dyspnœa at times came on, lasting three-quarters of an hour, during which he became livid. Again I urged bleedings to eight to ten ounces, but he declined to allow it to be done. He continued to grow worse, with accesses of great suffering, and finally copious hæmoptysis. It seemed to me now as if nature were trying to show us, by this very symptom, what should be done, but we foolishly, as I think, shut our minds to the suggestion, and decided not to follow her counsel. The cough finally became less violent. A little delirium at last came on, and he died 24 hours afterwards. At the autopsy next day, *much fluid blood was found in*

the heart; the aortic valves were rough and insufficient, so as not to hold water; enormous hypertrophy; effusion of fluid into both pleural cavities. The lower lobe of the right lung was solidified, partly with red "hepatization" and partly with distinct "pulmonary apoplexy." In the vessels of the last were found emboli, possibly derived from the vesicle plexus where there was a broken down coagulum. The liver was slightly enlarged, and of a nutmeg aspect, of yellowish hue.

As I look back now upon the circumstances of this case, I think there was never greater folly displayed by any physician than by myself and my comrade in not *urging* venesection at our last visit. If we had only taken the motto of our Society as our guide, we should have argued somewhat in this way: Again by folly this man has put his diseased heart to performing more than it can easily do. Nature is vainly endeavoring to relieve his distress by pouring out blood in her dull and distressing way, from the pulmonary vessels, and almost choking the patient while doing so. Follow, therefore, her suggestion and open a vein and give relief again. Instantly the question presented itself: Will you continue to bleed every two weeks in a case of organic disease? If so, you put your patient in the same category as that of the young girl whose case is given in the first part of these remarks. I mentally answered this suggestion by saying — Sufficient unto each day are its duties. The two cases are not analogous. For the first, when she came under our treatment, had been bled every two weeks for two and a half years for *non-organic disease.* This latter person has organic disease, but he has been bled once only, and with great relief. He has behaved foolishly, and brought upon himself another attack. Perhaps venesection will give relief again, and after that, perhaps he will learn

wisdom, and may live three-score years and ten, even if he have aortic disease and hypertrophy. I know of some now, who have been doing all the duties of an active life for years, with more organic trouble than he has now. So I mentally argued, and yet I left him to his fate. It is true I suggested the propriety, but I spoke to unwilling ears, and had not enough confidence in my own opinion to convince my opponents. I subsequently found that the patient himself declined to submit a second time, although he had joyfully consented to the first venesection. With these reflections, I pass to my last case.

Case IV.

It seems to me that death occurred in this case in consequence of a great obstruction to the movements of the heart. It became, consequently, *filled with blood* which it could not send forward. Venesection was not permitted. I regret this extremely. I believe that possibly life might have been saved by it, although it is true that some circumstances connected with the nature of the disease may seem at first sight to contra-indicate such treatment.

Dec. 27, 1870, I saw in consultation with Dr. ——, a gentleman of this city. He was seventy-two years old. He had always been in good health, except that for some time he had had a little dyspnœa on going up stairs. He had occasionally said at such times that he thought he had some heart trouble. But he had never had any such marked signs of cardiac disease as to need a physician.

About ten or twelve days before I saw him he had driven several miles into the country and was fully exposed to a very cold wind. He returned home much chilled, and feeling as if he had taken cold. He however was able to go

to his business for a day or two, but was never well afterward. He remained at home from the third day, with oppression about the chest. This was at first slight, but it rapidly augmented, and finally obliged him to sit up and to lean forward in order to breathe at all. Meanwhile there was no marked pain anywhere, and only a slight cough. There was a little crepitus one day in the middle of the left back, which the next day had disappeared, and instead of it there were some sonorous râles in other parts of the lungs. Meanwhile the dyspnœa increased fearfully, with total inability to lie down day or night. The pulse had become small, and rose to about 100. The circulation in the extremities had gone on worse and worse, and before I saw him he was pulseless in the radials, and the hands and feet were cold. The sputa were rather viscid and rusty, one day. The sounds of the heart had been obscure, but not manifestly morbid. He had been sallow in the early part of the attack, and had taken blue pill with free dejections as the result. Subsequently he took nitre and aconite 2 drops five or six times daily, and finally he had used ammonia and carbonate of ammonia with brandy and champagne, and had inhaled oxygen.

I saw him in consultation on the evening of the 27th, the eleventh day of the disease. His mind was perfect. He was sitting leaning forward, evidently suffering greatly, with a sense of distress in the chest. He complained of no other symptoms. His feet were swollen, and pitting on pressure, and his legs up to the knees, with his hands, were of an icy coldness. The heart sounds were very obscure, and the impulse scarcely, if at all, felt. The percussion was difficult, owing to his obesity and erect position. There was no positive evidence of renal difficulty. There was simply evidence of some disease which had caused a great obstruction to the circulation of the blood, and the fact that the legs and hands

were cold and livid seemed to indicate some serious blocking up of the heart. The weak impulse and sounds were those of a heart that could not easily move. The attack was acute. I thought that the heart, being thus distended, and consequently obstructed in its free movements, called for venesection for relief. No remedy had thus far done any good. Death was imminent, unless speedy relief were obtained. Should venesection be tried, was the question of questions. He was a large, full-blooded man. Evidently he could easily lose a few ounces of blood without fear of injury. I remembered the old maxim — "a doubtful remedy is better than none." I felt assured that the old physicians would have bled him long before I saw him. My position, therefore, in the consultation room, was that venesection should be immediately attempted. I could not advise anything else till that was tried. This was finally decided upon. Unfortunately having heard of some evil results, as he thought, from venesection, in his early days, the patient declined to have the operation done. And we, his attending physicians, had not faith enough in the proposed remedy to be willing to urge it, and to perform it or leave the patient as one refusing to submit to proffered treatment. He died in less than ten hours after my visit, having retained his consciousness to the last.

Autopsy, 10, A.M., 29th. Body fat; abdomen large, and with thick layer of fat on the parietes. Fat also about pericardium. On opening this sac, a thin patch of vivid red and recently effused lymph was seen on the left ventricle. A small quantity of opaque semi-purulent fluid with a little shreddy lymph was in the sac. *The heart was large; the right cavities were filled with dark, soft, semi-coagulated semi-fluid blood that could be partly scooped out with the hand, while other portions of it flowed freely away.*

There was much more of it than usual. In the left ventricle was also blood in considerable amount, and towards the apex was a coagulum, large, fawn-colored, and firmer than the rest, and this was intimately blended with the columnæ carneæ. The heart was very fatty, so that it was difficult even to lift it without its tearing. It broke readily under the finger; the valves were quite well. The lungs were œdematous throughout, but not otherwise diseased.

Several interesting points for discussion naturally arise on this case. Let us look at the precise condition of the organs and consider what was the cause of death, and what organ was mainly at fault. If we trace the history of the disease, and look at the post-mortem, we find a man who had had, for some time, slight cardiac symptoms, but no severe ones indicating serious organic change. This man drives, in an intensely cold day, many miles in an open buggy, as he often had done previously without any injury. But on this occasion, the weather having been very cold, he had become thoroughly chilled, and felt, to use the popular phrase, that he had taken a violent cold. Soon slight oppression about the breath was noticed, but no serious lesion of the lung was found. Suspicions of pneumonia and of bronchitis followed each other rapidly. Meanwhile, however, the pulse became very small, and the dyspnœa increased to a terrible degree, so that in about eleven days from the day of the drive, I found him suffering permanent orthopnœa, with the head thrown forward, as in organic disease of the heart. Coarse mucous râles were heard at the bases of both lungs. He was pulseless at the wrists. The heart was beating obscurely, but without morbid murmur. The legs to the knees, and the arms to the elbows, were of a deathlike coldness. He was perfectly conscious. Breathlessness and pulselessness were the prominent symptoms. The physical signs were

great obscurity about the heart, with marks of œdema of the lungs at their lower parts and of the extremities. It seemed to me as if the patient were dying from almost entire stoppage of the circulation. From the obscure movements of the heart, *I inferred that probably it was crammed with blood, which it was vainly trying to throw forward.* The conclusions to be drawn were as follows: 1st, that unrelieved, the patient would die within a few hours; 2nd, that the probabilities were that the heart was at fault, and was then laboring in vain to contract upon the mass of blood in it, and that our only chance of giving relief lay in removing some ounces of that blood, thus relieving the organ of its *extra* burden, and allowing it to contract upon its diminished contents; 3rd, I could see no possible objection to trying this remedy. The sole fear in my mind was that it was too late, and that no blood could be got from the arm. If blood could be made to flow from the vein, I had no doubt of at least temporary relief, although perhaps he might die afterwards, and I strongly urged the venesection. As I look back upon the case, I regard it as one in which medicine failed of its high functions, viz. of "curing" a patient. I use the word in its original signification as the act of "taking care of" a patient. Certainly with the results of the autopsy before me, viz. a very fatty heart, I could not hope to remove that condition, but we might possibly have removed an incubus that rested on the thus weakened organ. The patient, a hale, hearty old man a week previously, might have been restored to his usual health, and his debilitated heart might possibly have been put in the condition that it was before his exposure. But here arises the question: Is fatty degeneration of the heart *per se* an insuperable obstacle to venesection? I ask this question because this state of the heart of our patient has been cited as proof positive

that venesection would have done no good, and was wholly contraindicated in the actual case. But this is, as it seems to me, a begging of the question. On the contrary, even if we could be sure, in any case, of the existence of a fatty degeneration of the heart (which with our present means of diagnosis I believe we never can be fully sure of), I deny that that diagnosis contraindicates venesection. I ask the proof that I am wrong in taking this position. There are, in truth, no data wherefrom we can definitely answer the question, either affirmatively or negatively. Meanwhile, in support of my own views, I submit—1st, that other things being equal, we would prefer to bleed a full-blooded man as our patient was, rather than the lean Cassius-faced invalid we sometimes meet. But 2d, it may be urged that such an argument is in vain, because the fat in this case has usurped the structure of the heart, and thus has weakened its muscular powers. I admit it. But this argument acts both ways, and as much for my side of the question as for that of my adversary. I certainly would not take blood from a debilitated heart with the hope of giving strength to it, although Dr. Reid's* experiments fully prove that the removal of blood from a distended heart does give not only the opportunity for contraction, but actually stimulates the organ to renewed vigor of contraction. I therefore can see no objection to venesection in the case of a fatty heart, overwhelmed, so to speak, with *extra* work, but rather a greater reason for its use. We relieve these deteriorated muscles of their extra labor, and at the same time give them a healthy stimulus. Especially should we urge the operation when, as in the case related, the organ has had, from some exposure, the blood driven back from the superficies of the body,

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* See Dr. Reid's paper, quoted below.

and owing to the softening it has become oppressed by the extra quantity thus thrown suddenly inward upon it, and becoming stuffed full, cannot send the blood back into the extremities. Arguing, *a priori*, one would think that venesection would be our first thought, in order that we might quickly withdraw some of the offending fluid. For we should thus allow the wearied and overburdened organ the time and space required for its contraction upon what remains of the blood, and enable it to send again warm fluid in free currents into the pulseless and cold extremities. This, and and this alone, gentlemen, seems to me "Rational Medicine" in such a case.

On the contrary, the whole modern system of saturating a person so situated with large doses of liquor, or temporizing with various remedies, as we are so apt to do, seems to me, in not a few cases, fraught with real mischief, until the first step towards a proper "curing" of the patient, viz., venesection, be performed.

I have thus, gentlemen, given you some of my views of the abuse and the proper use, as I deem it, of this treatment by *venesection; a remedy potent for mighty evil, and also for mighty good.* There is nothing good in this world which, indulged in too freely, does not become bad. Evil is good run mad. Forty years ago venesection, as practised by every one, was an unmitigated evil. It was then tried in the balance and found wanting. Forthwith all the world said, "We will have nothing to do with it hereafter," and at the present hour we stand upon the following equally pernicious and unreasonable determination, viz., that for fear of falling into the habit of over-venesection, we will not bleed at all. Can there be any conclusion more absurd?

Meanwhile we are using other remedies with a recklessness quite equal to the venesection of former days. Subcuta-

neous injections of opiates and of other narcotics, and the internal use of the various forms of alcohol, are common everywhere. And if I mistake not, the results to the community of this over-use of these various remedies, will be at times quite as injurious to human nature as the over-bleeding of a former day. In this country we are peculiarly irrational in these respects, for, forgetting the differences of climate of the two continents, and the modifications the Anglo-saxon race undergoes on this side of the Atlantic, we have adopted much too freely, in my opinion, the ultra doctrines of Todd. I would neglect neither of these classes of remedies, neither blood-letting nor stimulants, but I would use both rationally and with a clear judgment of the necessities of each individual case.

If I mistake not the signs of the times, the position I now take will commend itself, gentlemen, to your sober second thoughts. At any rate, I rejoice to find that I am sustained by such eminent men as Dr. Richardson, President of the Medical Society of London, and Dr. Sutton of that metropolis, Dr. Fordyce Barker of New York, and Graefe of Germany, men of extensive repute and of practical good sense. Moreover, in conversation with Drs. Brown-Séquard and Lombard, I find that Modern Physiology fully sustains the views I have advanced. I will conclude what I have to say further by giving extracts from these various writers.

Dr. Richardson says,* "If blood-letting were in this day an unknown remedy, and were some man to discover it, we should receive that man as the greatest amongst us and send him to posterity as one of the lights of the age." And again he says, "The confidence of the ancients in the prac-

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* Address introductory to the ninety-fifth session of the Medical Society of London, on Blood-letting as a point of scientific interest, by B. W. Richardson, M.D., F.R.S., President. (Practitioner, No. 5, November, 1868.)

tice of blood-letting, their fearlessness of any immediate danger from it, was, I believe, as well founded in truth, as the cowardice and assertion, without observation, of the present day is founded on error." He sums up as follows: "I would recall that blood-letting as a point of scientific practice is still open to us in some stages (early stages) of typhoid fever, in cases where there is a sudden tension of blood, of which sunstroke is an example; in cases of chronic congestion of the brain; in cases of acute pain from (inflamed) serous membrane; in some cases of spasmodic pain (gall-stones, &c.); in others of sudden arrest of circulation from concussion; in congestion of the right heart, and it may be in some cases of extreme hæmorrhage. Above all I claim for it a first place in the treatment of simple uræmic poisoning."

Dr. Sutton (Medical Times and Gazette, Dec. 18, 1869) gives cases in which bleeding was resorted to, to relieve distention of heart and passive congestion of the lungs. He ordered it, not to relieve inflammation but to cure obstructions.

Fordyce Barker, M.D. (N. Y. Medical Journal, Jan., 1871.) "I am gradually getting to bleed more frequently. My conviction that this resource in practice has been too much neglected by myself and others has been progressively growing for some years." Dr. B. would bleed to prevent abortion in some cases. So in renal congestions of the brain with coma and when the skin is hot, there is nothing so sure. He would bleed a woman in convulsions thirty ounces, and give elaterium also. "We must not," he also declares, "avoid bleeding in some cases even if the patient is feeble. In puerperal mania, at least in some very rare cases, venesection is of the greatest benefit.

By a recent lecture delivered by one of our younger

associates (Boston Medical and Surgical Journal, Vol. vii., N. S., page 363), H. Derby, M.D., I learn that the famous Graefe of Berlin, the eminent oculist, relies very much on venesection, decided but not severe, as a means of warding off the serious inflammatory results arising sometimes after the extraction of the lens.

Finally, in support of my views, I gladly quote some experiments made by Dr. Reid some years ago.* Dr. Lombard, of this city, draws my attention to them as amply sustaining all that I have claimed. I think I am right in declaring also that Drs. Brown-Séquard and Lombard are fully convinced that modern physiology condemns emphatically the present opinions of the profession, whereby venesection is virtually abandoned. Physiology says that we are all in grievous error in this matter. Dr. Reid states, in his essay entitled "On the effects of Venesection in renewing and increasing the heart's action under certain circumstances,"† that he has observed in several experiments on the lower animals, that disgorging the right side of the heart, when its contractions are enfeebled or suspended, by opening the external jugular, has, in some cases, a decided effect in renewing its action, and this, he is convinced, may be of considerable practical advantage in promoting the return of the circulation under certain circumstances.

He would open the jugular in cases of asphyxia and let the blood flow only from the lower opening of the vein, so as more quickly to remove the blood. Again, there are cases in which stimulants will be of no avail till after venesection.

In conclusion of these remarks, I desire to indicate some

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* Physiological, Anatomical and Pathological Researches, by John Reid, M.D. Edinburgh, 1848.

† Essay Third of above named Researches, page 51.

of the cases in which I think that venesection should be performed.

First—In all acute or chronic cases where from any cause the heart apparently becomes distended with blood and consequently the circulation is greatly impeded, whereby orthopnœa, lividity of the skin, with a very feeble, small and generally rapid pulse, mixed perhaps with other serious and distressing symptoms, are produced.

Second—It seems not uncalled for when there is a very acute pain in any part of the thorax, for example, from inflammation of the pleura, causing orthopnœa and distress, even if there be no obstruction of the heart's action. The question has often arisen in my mind whether the many cases of large pleuritic effusions which we see now-a-days, may not be due, at least in part, to our neglect of venesection during the earlier and more acute period of the disease—when pain in the side is quite severe, and all the symptoms are very marked and the pulse and temperature are increased.

Third—In violent acute cephalic symptoms, threatening serious results, when the head is hot, the face flushed, and the pulse is full and hard.

Fourth—In certain cases of threatened miscarriage, occurring at certain times during pregnancy, I know of nothing better than venesection to check the tendency. Even after hæmorrhage from the womb had actually commenced, and even when it was attended with expulsive action of the uterus, I had, many years ago, a case wholly relieved by venesection to the amount of a few ounces only.

Doubtless other cases than those that could be included in the above summary, will arise in the practice of most physicians. I only quote the above as examples of classes in which I have no doubt of the benign effects of venesection. That blood-letting is destined again to be used by the profes-

sion, I have no doubt. We shall hereafter use it "rationally." Remembering the extravagances of our fathers, we shall avoid, I trust, those follies in the application of this most powerful remedy which have so paralyzed us of the present day. That paralysis, as I sincerely believe, allows some patients now to die who might be saved by the *Rational Use of Venesection.*

THE PRACTICAL

ASPECTS OF MEDICAL SCIENCE.

By JOHN DOLE, M.D.,
OF AMHERST.

READ JUNE 7, 1871.

THE PRACTICAL ASPECTS OF MEDICAL SCIENCE.

Since the days of Sydenham, practitioners of medicine have been divided into two classes—scientific, and the so-called "practical" men. The distinction is an altogether arbitrary one, but, through constant reiteration, has come to be looked upon as valid. Members of the profession, sometimes wittingly but oftener through carelessness, have adopted the distinction, as they have winked at the term "allopathy," endorsing the false note and giving it currency, greatly to our own detriment. The injury might have ended with us, in such looseness of thought as the vague use of terms always begets, had it not opened the way for misapprehension on the part of the laity concerning the abilities and duties of doctors. This misunderstanding is much more wide spread and pernicious than would appear at first glance—amounting to almost terror of science on the part of the ignorant, and in the minds of the more intelligent to a belief in a supposed inherent distinction between scientific men and "family physicians." The public mind, always inapt in respect of nice discriminations, cannot dissociate science from mere theory, hence have arisen the whole catalogue of slurs, inexpressibly foolish when applied to rigid students of the phenomena of disease : "impractical," "wanting in common sense," "book-worms," "vagarists," *et id genus omne.* To a close observer and thinker, however, the broadest distinction exists between science and vague theory, and what

I have to say to-day is in illustration of the proposition that SCIENCE IS NOT OPPOSED TO PRACTICAL RESULTS IN MEDICINE, BUT ON THE OTHER HAND ITS RESEARCHES CONSTITUTE THE BASIS OF ALL TRUE AND PERMANENT ADVANCE IN THE ART OF HEALING. It may be that, from a candid consideration of this subject, we shall come to better understand our daily obligations to science, and that some corollary for practical every-day use may suggest itself.

The "Art of Healing" considered abstractly and in exact definement, presents itself as a problem composed of two elements: the "Natural History of Disease," and the exact physiological action of means of relief or cure—such means including not only articles for topical or internal exhibition, but all constituents of the Materia Medica—Subjecta, Ingesta, Circumfusa and Medicamenta.

The first element, that of "Natural History," is the resultant of many factors: cause, duration, seat, temperature and pathology, itself a factor capable of subdivision, while our relation to this element finds expression in diagnosis and prognosis. The value of this relation to ourselves and to our patients depends primarily upon our appreciation of all these factors, the methods of their determination, their exactness, positive and negative characteristics, limitations, &c., rather than in any ignorance or unqualified assumption of them as parts of a proved proposition. As a rule this appreciation evinces itself in our justness of bearing towards experiments and researches looking to the elucidation of these points—discoveries in anatomy, normal or morbid, researches in chemistry and physiology—improvements in general surgery or elaborations of its special departments—the eye, ear, uterus, larynx, venereal disease, dermatology, &c.—in short, towards Science in any of its workings and manifestations.

Beginning, then, with the subdivisions of our first element we find that the *causation* of diseases, especially those of the zymotic type—always the object of guesses more or less happy—has of late been subjected to rigid investigation. Typhus fever yields to patient observers some secrets of its predisposing and exciting causes, and to drinking-water impregnated with organic impurities may be ascribed the especial severity of some outbreaks of cholera. That at St. James Westminister, London, in 1854, owed no small part of its fatality to the water of a well in Broad Street, "famous," says Dr. Anstie, "for its sweetness and freshness." Repeated analyses of the water by Dr. Snow, demonstrated the presence of these impurities, and discontinuance in the use of it was attended with immediate abatement of the disease. So also the development of cholera among the inmates of the houses supplied by the "Lambeth," or "Southwark and Vauxhall Water Companies" respectively —the mortality (even in the same street, and in houses directly opposite one another) being in exact ratio to the amount of organic impurities in the water used.

Again, the exciting cause of typhoid fever has been even more carefully and laboriously determined. The war now raging between Murchison and Dr. Budd, is a war of words merely to determine which link in the great chain of cause and effect shall be considered "immediate causation." The tide of battle surging between the maintainers of the "pythogenic" theory and its opponents, leaves at each ebbing a deposit of scientific truth—the outcome of rigid studies, exact calculations, innumerable chemical analyses of water and the vapor of sewage—all of which serves to lift Etiology above the plain of happy guessing to that of a science—a science not all-embracing nor as yet very broad, but close and compact, to which much may be added but from

which nothing shall be taken away in the coming years. And this science finds expression in such sanitary and preventive measures—cleanliness, deodorization, pure water and drainage, as make disease less fearful and more manageable, epidemics less prolonged and fatal. Surely this is practical.

Knowledge of the *seat* of disease—the exact organ or tissue affected, and the liability to complications more or less serious—increases *pari passu* with advances in medical science. Observations of the external phenomena of disease, if unsupported by rigid tabulation and comparison in the milder cases, or in the severer by post-mortem verification, would leave us still in the open field of conjecture. It has fallen to the experience of the youngest of us to learn that symptoms the most prominent and obtrusive often prove unimportant, or of but secondary significance; brain symptoms in carditis, diarrhœa in pneumonia, hydrocephaloid in the acute enteritis of children, spasm of the glottis at the onset of rickets, albumenuric retinitis, irregular action of the heart, or dyspnœa in organic disease of the kidneys. So, too, the "signs," that for want of clear comprehension on our part have been styled diseases, resolve themselves latterly, under nicer study, into mere evidences of a broader underlying cause. Vague terms are fading from our nosological tables; special plans of treatment with reference to stomach or spirits are abandoned; liver diseases in adults, and worms in children, are playing subordinate rôles, and "slow," "infantile remittent," "low," "gastric," "bilious," and "continued" fevers are disappearing, swallowed up in that broader condition known as typhoid, whose seat and pathological concomitants are known with even microscopic exactness. Such recognition of these symptoms and complications as simplifies our nosology, and so relieves mental

confusion, tends to render clear and easy diagnoses otherwise impossible, and puts treatment, if treatment other than hygienic be deemed necessary, upon a more rational basis. This, too, has a "practical" aspect, as has also the introduction of the element of *temperature* into our calculations. The thermometer, though at first unduly praised and criticized, like other adjuvants in investigation has gradually asserted and maintained its claims to notice—claims based upon nothing so little as guesswork, or even upon statistics imperfectly interpreted, but upon critical observations now almost innumerable—those of Wunderlich at the Liepsic Clinic alone amounting, in the year 1865, to more than 600,000. Over and over again these observations and the deductions therefrom have been verified by Virchow, Griesinger and Traube, in Germany; by Becquerel, Bernard and Breschet, in France; by Parkes and Jenner, Bennett and Anstie, at Edinburgh; at Guy's, St. Thomas's, at Westminster, and later by the leading observers of our own great Hospitals. While these observations throw but little light upon the question of the absolute cure of disease, they offer, as practical outcome, proof that preternatural heat is a constant and essentially dangerous concomitant of fever, and that rigor is a wholly subjective phenomenon, dependent on the condition of the peripheric nerves and not accompanied by any decline in temperature. They have taught us the typical laws of particular forms of fever, "supply a basis by which to determine whether any individual case is progressing as it ought;" enable us to anticipate the onset of fever in puerperal women; to prognosticate with certainty paroxysms of ague; afford a means of absolute differential diagnosis between typhus and meningitis in cases otherwise obscure; point inexorably to the thorax in acute tuberculosis in the absence of cough, expectoration, or even hurried res-

piration, and in the exanthemata enable us to forestall, by palliative means at least, the days of greatest discomfort.

Of Pathology, the last, and possibly the most important subdivision of our first element, we may speak but briefly. The light thrown upon the "Natural History of Disease" by researches in this department of study, enables us to posit a line of demarkation between the curable and essentially incurable — a line varying somewhat in its individual increments from year to year, but which always holds a place consistent with all known data — and even anticipates, by position and tendency, new developments in therapeutics. It proves that *inflammation* is but modified nutrition, of which pain, heat, redness and swelling are the accidents and not the essence; furnishes us, to dwell only upon the more obvious matters, with exact data concerning healing of wounds by "first intention" and by granulation — the granulations of ulcers — the anatomical peculiarities of the walls of abscesses; tells us what tubercle is, what it indicates and how it tends to fatality wherever found; tells us the how and why of the duration of certain diseases, and indirectly teaches us what Nature can and will probably do — at the same time suggesting, by implication at least, the proper moment for interference or the judiciousness of withholding our hands. Viewed from the surgical side it affords still more important revelations concerning the healing of fractures, the function of the periosteum, diseases of joints and foreign bodies therein — anchylosis, curvatures of the spine, caries and necrosis and the nature of tumors. How immediately practical such revelations may be in their bearing on wise procedure, no one of us can have failed to observe. In truth in tumors, what concerns us chiefly, perhaps wholly, is their *tendency* rather than their topical or plastic manifestations (excepting always those outgrowths, which, though by

nature innocuous, tend to fatality through rapid recurrence). Vascularity to aneurismal thrill, or non-vascularity and density to the appearance of cartilage, rapidity or slowness of growth, excess or deficiency of fibrous, cellular, glandular, serous or epithetial tissue, are but the inconstant local peculiarities of any outgrowth under whatever form occurring. The one significant fact concerning it, *cœteris paribus*, is the presence or absence of some cell which fails of correspondence with the ultimate element of any tissue recognized as typical. This cell, never so much as imagined after centuries of study and observation in the gross, stamps, in the present state of therapeutics, the seal of fatality upon all such outgrowths as contain it in great numbers, and proves to be the constant outcome or cause of a malignancy of which all other conditions are the insignificant concomitants. This is the one practical matter for us to know concerning tumors, for on such knowledge must be based, if it would be wise, our treatment and prognosis. So much for our first element of "Natural History."

The stock criticism urged against us as a class has of late assumed the form of a complaint that though we are coming to know more of what disease *is*, we fail to advance proportionally in our knowledge of the means of cure. The criticism is specious, but neither altogether true nor sound, since the very knowledge ascribed teaches us, as we have seen, the essentially incurable nature of some diseases — restrains us — tells us that many formerly supposed cures could have been but coincidences, and suggests caution in the exhibition of our remedies. The new light thrown by science upon one element of our problem of cure, even when faint, sometimes serves only to bewilder us, as morning or evening twilight blinds and bewilders men long immured in dark dungeons. As an effect of this knowledge,

resulting in an enlightened skepticism, Chomel's rule of "doing nothing when uncertain what to do," is usurping in wiser quarters the old English and American practice of when doubtful giving a dose of calomel—a result of incalculable advantage to our patients. But this negative virtue has its complement in positive acquisition. We know, as the result of chemical and physiological investigation, more than formerly of the action of certain drugs—their time of absorption and elimination—duration of action and reaction; that gallic acid does its work and is eliminated in two hours, acetate of lead in not less than six or eight hours; that the seemingly safest dose of turpentine, too large for stimulation of the primæ viæ, too small for catharsis, in spite of its conservative name of "medium," is the really dangerous dose. Specialists have taught us much to be followed and to be avoided in the employment of certain remedies—Hydropathists of water, Oculists of belladonna, Venerealists of mercury and iodide of potassium, Dermatologists of arsenic and the external application of alkalies. Brown-Séquard teaches some, and suggests more, uses of the bromides, by first showing their physiological action on the blood-vessels of the nervous system. The science of chemistry in one of its exactest attitudes gives us the antidotes of poisons. Out of a German laboratory came the inspiring whisper of the possible therapeutic value of chloral hydrate. Parkes and Wallowicz, Schulinus of Dorpat, Anstie and Dupré, are revolutionizing our belief in alcohol; Radcliffe, working from a different stand-point, confirming the latter observers as to neuralgia and its appropriate treatment by gentle stimulation, and within a few years Briquet has solved for us scientifically and almost exhaustively, the action of Peruvian bark and its alkaloids. Practically, our brothers came back to us alive and in health from Southern swamps, where every

exhalation was pregnant with disease; and only months ago, around Metz, following, as he himself testifies, the hints afforded by laboratory experiment on the antipyretic action of quinine, Prof. Binz brought back to life German soldiers dying of septicæmia, as if by some miracle of healing. Indeed, there is now, not opposed to but supportive of the practice of therapy strictly empirical, a science of therapeutics on whose teachings, consciously or unconsciously, each one of us relies with a degree of assurance in his attempted control of hæmorrhage, in his choice of a remedy for the inducement of sleep, for mastery of the miasmata, and in the treatment of all diseases characterized by periodical exacerbations — a science not more responsible for the pretensions of quack panaceas than is this Society for Townsend's Sarsaparilla or the Russia Salve.

Let us pause here for a moment to see how and where we stand. Practicality or Empiricism claims for itself the distinction of resting on a basis of facts. Well, Science, through its disciples — philosophers, general practitioners, or specialists — working patiently and with catholic tendency, discovers facts on every hand. More than this, it collocates, groups, and arranges them, and furnishes us with principles to which those now known and all subsequently discovered facts can be referred. Therapy looks to method, rather than to isolated observations, in the study of drugs for its greatest advances, and the profession owes much of its knowledge of the "natural history" of disease to accurate and systematized investigation, which is Science. The especial advantages, negative and positive, of such knowledge, manifesting themselves in non-interference no less than judicious treatment, suggest as a practical deduction the primal and essential importance of diagnosis. Improvements in treatment, better statistics, smaller percentages of death other

things being equal, marked sanative results, follow thorough comprehension of disease in such direct causative relation as to defy the suggestion of accident or mere coincidence. Indeed, approximation to nearness of understanding of the disease in question, and only this, renders possible the sure aiding of nature by enforced sanitary measures, or otherwise. Complement such knowledge by clearness of view of some therapeutic means, and our cure-problem, if in the nature of things resolvable, becomes comparatively simple. The most critical examination may leave us to conjecture after all, but to a *reasonable* conjecture, and not to the flippant, arrogant guess-work that fails oftener than it succeeds. "The conjecture which guides the physician," says Dr. Latham, "is rigorous and calculating and honest. It acts strictly by rule, and leaves nothing to chance. It does not see the thing it is in quest of, for then it would no longer be conjecture. But because it does not see it, it ponders all its accidents and appurtenances, and noting well whither they point, it takes aim in the same direction, and so oftener hits the mark than misses it. And succeeding thus, it knows why it succeeds, and it can succeed again and again upon the same conditions. Next to knowing the truth itself, is to know the direction in which it lies." This is the peculiar praise of a sound conjecture, and any assistance, instrumental or other, tending to render such conjecture possible, should not be neglected. All adjuvants in medical diagnosis, as in other departments of science, incur the double risk of undue laudation and distrust. The solar and chemical microscopes not long ago, and lately the thermometer, suffered apotheosis — were so bepraised as to excite all conservative minds to ridicule, till Dr. Holmes's "Stethoscopic Fly," exceptionally an entity, was claimed under imaginatien as existing everywhere — the reactionary wave of skepticism

in its turn rolling beyond the point of rest. But the oscillations ceasing, no wise man ignores, in critical examinations, microscope or stethoscope, ophthalmoscope or thermometer. "They are but artificial and refined means of scrutiny. They cannot enlarge human capacity nor strengthen human understanding;" but beyond the reach of human eye or discrimination of human ear or accuracy of human touch, they find their proper work, bringing within cognizance objects and elements otherwise unknowable. No most delicate musical ear, unaided, can always distinguish the fine crepitus of incipient phthisis from the râle of capillary bronchitis, yet a world-wide difference lies in the treatment and probable prognosis. The hot, dry, pungent skin, at the onset of scarlet fever, coexists with a temperature of 102° Fahr., while the softer, moister, and less burning feel of the climax, is coincident with 105° or 106° Fahr. "It is not," said Lord Bacon, "the insufficiency or incapacity of man's mind, but the remote placing thereof that breedeth mazes and incomprehensions; for as the sense afar off is full of mistaking, but is exact at hand, so is it of the understanding—the remedy whereof is not to quicken or strengthen the organ, but to go nearer unto the object." This our instruments enable us to do, and so serving should be employed, judiciously, as subsidiaries, not substitutes, when unaided sense fails. All power, every instrument, all knowledge of each and every branch of medicine, all faculty of clear or logical reasoning, should be brought to bear upon our diagnosis. No supposition should satisfy when exact knowledge is attainable. No appeal to a limited personal experience is justifiable when the records and deductions of combined experiences are accessible. No amount of time or patience can be considered superfluous until the problem is solved or is proved too intricate for solution, and, whatever treatment

may be under the light afforded us through such critical inquiry, and it is often confessedly faulty, without it it is altogether pretentious, impertinent, shallow.

But hypercritical attention to unimportant details is sometimes mistaken, by performers and bystanders alike, for scientific thoroughness. Elaborateness is not of necessity accuracy, though it is often the wearisome concomitant of accuracy, and the nicest discrimination of symptoms is perfectly consistent with rapid generalization. A moment's touch of the radial artery determines for the trained brain its quickness, force, rapidity and volume, though no wise look be assumed or watch consulted. The record books of great hospitals often furnish an example in point of how much can be written, how many observations — each elaborate and accurate in itself — can be recorded without furnishing one with any adequate conception of the appearance of patient or a hint concerning diagnosis, prognosis, or treatment. It is wearisome to remember how the memories of students are overloaded with details — daily records of chest measurements in cases of pleurisy, where the question of effusion had been determined beyond the shadow of doubt; records of pulse in diseases whose natural history shows every variety of cardiac action; records of dejections always normal; records of urine tests where no kidney involvement was to be feared; records of sleep in hours and minutes, where sleep, or the absence of it, within wide limits, was utterly irrelevant to the treatment or issue; examinations of blood-corpuscles in short-lived functional derangements without suspicion of previous organic or zymotic disease — and so on, if not *ad infinitum* at least *ad nauseam*, the salient cardinal points of the cases either forgotten or brought down to the dead level of ordinary symptoms. This is the disease of Science — the cant of its sham disciples who substitute

shadow for substance, who seek to comfort pneumonic patients dying for want of proper food and stimulus, by the assurance that their urine has been frequently and critically tested for the chlorides!

The most patient and rigid study for diagnosis, if it be wisely honest, is conscious of conditions beyond its ken—of knots of symptoms where the most pertinent revelations seem tied up the tightest—of points that defy scrutiny. Moreover, it is conscious of exigencies that demand immediate action on the basis of their broader features; hæmorrhages that must be checked at any hazard; attacks of pneumonia presenting such complete and overwhelming congestion as to demand the lancet in full view of the long convalescence or even possible death from asthenia; spasmodic colics, where in wisdom no cause may be sought, but where pain demands instant relief—in short, cases where all attempts at study are as foolish as they are ill-timed. Treatment in such emergencies, far from being rationalized, is only crippled by inquiries into the scientific aspects of the case. "The real physician," said Broussais, "is the one who cures. The observation which does not teach the art of healing is not that of a physician, it is that of a naturalist." Still, such cases, necessarily occasional, should not render us illogical. They cannot invalidate the rule that we owe to each problem the deference of attempted resolution, any more than the birth of sporadic genius of insignificant parents invalidates the rule that like begets like. Nor do they detract an iota from the significance of Cullen's dictum "that neither the acutest genius nor the soundest judgment will avail in judging of particulars in which they have never before been exercised."

I would not be understood by these remarks in any way to decry or undervalue experience. More than two centuries

ago the wisest and most philosophical of then living men concentrated in a single clause all that could be said upon that point—"they be the best chirurgeons which being learned incline to the traditions of experience; or being empirics, incline to the methods of learning." Out of such union springs the highest curative skill, as naturally as out of the elements of earth and air, by selective capacity in root or tendril, and by a method which God knows, some consummate flower is elaborated. But all unrecorded experience, even when evidenced in daily practice, is necessarily exclusive—confined in its benefits to those in immediate contact with its possessor. Moreover it is by nature arbitrary, quite as often possessing men as they *it*. It has great value for us in the vast fields of learning and labor, yet intrinsically and alone is worthless. It is often crude, blind—leading men to false no less than sound conclusions; deals in the *post hoc* rather than the *propter hoc;* supplies reason with materials for work, yet is powerless to work; is accretive, prehensive—gathering anatomical, chemical, therapeutical, physiological data, as results of its labor. But just here the specific work of experience, as such, ends. Science objectively, or the methodizing faculty which is the subjective analogue of science, seizes upon these materials: inquires the how and why; rigidly examines each body of seemingly analogous or similar data; draws its lines of impenetrable distinction between truly logical deductions and specious though unsound inferences; systematizes, compresses, and makes portable what before was clumsy and useless through clumsiness; tabulates, formulates, coördinates; broods over this chaos of elements until a kosmos is evolved whose beauty and symmetry excite admiration, and whose use no sane man may deny. And these results, so elucidated and arranged; so interpenetrated by a method which allows the broadest

freedom of observation, while it binds us to rigor, and nicety and exactness of inference, enter in as an element of the later, and so wiser, experiential philosophy of all subsequent students of medicine, if they be students indeed. Who can doubt the practical worth of all this?

Yet this Science of which I have been speaking, in no wise encroaches upon individual thought or philosophy. It gladly bends its ear to the persuasions of genius — to all prophecies and intuitions that carry with them the stamp of reason. It knows well that McLeod suggested long ago in India its own present pet treatment of typhoid fever and dysentery, and reverentially remembers that Gibson anticipated, by more than thirty years, the surgical propositions established by the American and later European wars. It simply holds to the proposition that "the laws of nature are to be discovered by accurate observation and generalization of a sufficient number of phenomena, and not by any abstract philosophical mode of reasoning." Holding firmly to this ground, and recognizing fully the truth that the grace of prophecy comes to but few Sydenhams or Abercrombies, McLeods or Gibsons, it simply attempts to give to all data so proved and arranged as to furnish a basis for reasoning approximatively correct — leaving to the acumen and ability of each one of us the solution of the problems of individual cases; in its broadest and truest expression accomplishing what Hufeland declares its highest possible attainment — to *generalize* the disease and to *individualize* the patient.

If there be any truth in what has been said, the deduction is an obvious one. There can be no valid distinction between the Science and Art of Medicine, since it is the substratum and informing method of science that renders our daily practice other than a confused jumble of guesses, even artistic. All attempts at invidious comparisons between our

art and medical science are mischievous and to be reprobated. They have crept into our literature, and essays upon the "Art of Medicine," as something inherently distinct from the visible practical results of scientific training, have come upon us of late like the locusts upon Egypt, and with analogous result — working mischief in our thoughts, and paralyzing our great schools in their attempted reforms. The fact stares us in the face that our students seek other means of instruction than those furnished at home, while we who are in the whirl of active practice look to the incoming steamer for food for medical reflection. We but beg the question in stating or reiterating our belief that "it is the fashion," so long as the best and most promising and eager, those who have used faithfully every offered opportunity, feel most acutely their need of something more — so long as the very teachers of whom we are most justly proud, almost without exception, received their final instruction as students in London, Paris, Edinboro' or Dublin, or in later years at Berlin, Leipsic, Göttingen or Vienna. The record of the American students in Europe is one to be proud of, and the fashionable traveller in search of some easy road to medical fame, or the dilletante, has played but an unimportant rôle among them. They have shown equal capacity with their foreign brothers when equal opportunities for observation and study have been offered them. Our schools are realizing this, and trying to remedy their defects — encountering their greatest opposition in the careless attitude of medical men as manifested either in passive indifference to change, or in an active warfare on the ground of an imagined want of practicality in these movements; as though aught could be more practical than the training, and exercise in its self-appointed work, of the human mind, the centre whence radiate all practicalities. Surely it is useful to be able to distinguish

a peri-uterine tumor from a fœtal head—the dyspnœa of cardiac complication in Bright's disease from asthma. There are no diseases, save those of a strictly theological type, cured by wise or pompous looks, or even by the mere "laying on of hands," scrofulosis long ago having ceased to be amenable to the affluence of the royal touch. These reformatory measures, to be successful, need the coöperation of all medical men, and a full and hearty recognition of the fact that marked improvement in clinical instruction, or in therapeutical teaching, implies a more rigid ground-work of scientific training. Certainly the need is an imminent one so long as things knowable are as yet unknown, so long as the imagination must be invoked in explanation of causes purely physical, or the terms "spinal disease," "consumption," "bilious," "nervous," and "alterative," cover for us such a multitude of the sins of ignorance. "The human eye," says Carlyle, "sees in all objects that which it brings with it the power of seeing," and student eyes, trained in the results which science has arranged out of the invaluable but crude materials of united experiences, as expressed in best books, and in acute observations of the phenomena of bedside mysteries, growing achromatic at their work, see in new remedies, in all adjuvants of diagnosis, prognosis or treatment, not objects for ridicule on the one hand or mad overpraise on the other, but clearly defined desiderata, each with a value of its own, the measure of which only time can determine. It must be admitted that Science, studied either in a single salient aspect or in its entirety, can in no wise change the inherent power of its students, but it trains and moulds existing faculties. It cannot, as has already been said, "enlarge human capacity nor quicken human understanding," but it tends to elevate executive power to its maximum, and to reduce mistakes of belief and of action to the minimum.

"With *brains*, sir," said a famous painter, when asked with what he mixed his colors; and "with *brains*," I doubt not, would be the answer of any great physician or surgeon if asked with what he mixed his prescriptions. The science of optics may teach the painter the distinction between primary and secondary colors—the exact proportions that give in combination violet or brown or orange, but genius only can elaborate from these scientific details landscape or portrait. Chemistry will teach the student the laws of compatibles; Physiology, the broader laws of life; Anatomy, normal and morbid, the parts affected and how; still the ultimate appeal for adaptation of means to the ends of relief or cure must fall upon the brain of the prescriber—upon his personal sagacity. But the failure of any artist who should attempt landscape or portrait without some exact knowledge of the science of linear perspective and color, is as certain as to-morrow's sunrise. May not a like failure be predicted of any so-called Doctor of Medicine attempting cures without first studying and understanding, in so far as they are known, the laws of disease or the laws that underlie and control the substances and elements composing the broadest Materia Medica?

PARASITOLOGICAL INVESTIGATIONS

UPON THE

VEGETABLE ORGANISMS

FOUND IN

MEASLES, TYPHUS EXANTHEMATICUS, TYPHUS ABDOMINALIS, SMALL-POX, KINE-POCK, SHEEP-POCK, CHOLERA, &c.

BY DR. ERNST HALLIER,
PROFESSOR IN JENA.

Translated from the German,

WITH AN APPENDIX.

By Henry C. Perkins, M.D.

NEWBURYPORT.

1871.

TO THE FELLOWS

OF THE

Massachusetts Medical Society,

THIS TRANSLATION OF PROF. HALLIER'S BOOK ON THE "GERMS" OF CERTAIN DISEASES,

Is respectfully submitted, by

A Fellow.

INTRODUCTION.

BY THE TRANSLATOR.

My attention was first directed to the subject of "Disease Germs" by a work of Dr. Beale's bearing this title. After its perusal, which left the matter still open, the work of Prof. Hallier on "The Vegetable Organisms found in Sheep-pock, Kine-pock, Small-pox, Measles, &c.," was kindly put into my hands by a friend, Mr. Carl Meinerth. It was in an unknown tongue; but the interest felt in the subject led to the study of so much of the German language as would enable me to understand its contents.

I had already for some months enjoyed the acquaintance of a young German student, Mr. Carl Castelhun, who had been educated at Heidelberg, and who had made the study of the lower vegetable organisms a pastime in connection with his professional studies, and who besides was familiar with the use of the microscope. His services therefore were solitited and cheerfully granted in this new investigation. To him I am indebted for the correct rendering of the text.

The microscope used for our purpose was got up by Mr. Edwin Bicknell, microscopist at the Zoölogical Museum, Cambridge. Its optical properties have proved satisfactory, its powers ranging from 35 to 2750 linear diameters.

Some liberties have been taken with the original in omitting the translation of certain parts which were of an historic and polemic nature, or which had no important bearing upon the subject. All else has been translated, and I have thought it advisable to add, from another work of Prof. Hallier, a

description of his Culture apparatus, that such as may feel disposed to repeat his cultures may profit by his experience.

The hypothesis that there was something invisible and intangible which occasioned contagious and miasmatic diseases, it is well known, had been started in earlier times; and until now all medical men have believed and declared, that the causes of certain diseases, as measles, scarlatina, &c., were conveyed through the medium of the air. This doctrine, restored by Hallier and others, not only brings the bodies above referred to before us, but assumes to define for each different form of zymotic disease, the micrococcus (or yeast) of a specified fungus as the miasm or contagion.

This revived hypothesis, the doctrine of ferment, has met with an exceedingly favorable reception in Germany, and wrought a change in the views of many distinguished observers and physicians.

It remains now to test its truth, not by speculative reasoning, but by experiment and observation. Under direction of the British Government the initiative has been taken, and it is to be hoped the example may be followed, if not by other nations, by individuals willing to aid in discovering the truth in so far as relates to other diseases incident to man and brute animals, and the plants upon which they feed, and which are used as medicines.

I would only add, that in oder to understand my author's theory, his work on "Fermentation" was imported, and such parts studied as appeared necessary. Its translation must be left to some younger person.

ABSTRACT OF PREFACE.

In this work a division and contribution of labor took place which I have so often desired, and which is more necessary for an epidemico-logical than for any other question in Pathology. The previous history of Parasitism and Epidemiology proves this difficulty, that no inquiry into these two subjects can be satisfactorily carried on from the medical side solely; the co-operation of Botany, Zoölogy and Chemistry is required. * * * *

Dr. Zürn undertook the pathologico-anatomical part of this investigation, and myself the botanical, &c. A constant mutual interest and control was kept up by daily discussion. The publications are kept apart, so that herein only the vegetable organisms and their effects appear. Dr. Zürn will hereafter report upon his part of the labor.

Dr. Reiter, of Munich, most obligingly and faithfully assisted me by furnishing the material of cow-pock and that taken from the human subject. What I could find in it is given in the following pages. Furthermore, I add the result of the most recent investigations upon the contagion of cholera, for the material of which, from five different cases of Cholera asiatica and one of Cholera nostras I thank Prof. J. Vogel, of Halle.

Whereas, in cholera the chief argument, that fungus and contagion are identical, is with great difficulty supported directly, because so few cases are offered for examination, and the nature of the disease itself presents obstacles—so there remains only the second way of proving indirectly, that always the same organisms accompany the cholera. This method I have pursued, and as will be seen at the end of the treatise, not without happy results. The dejections of seven different patients have yielded in my cultures precisely the same results, viz., that in the cysts (fruits) micrococcus is formed, which must be considered as the contagion.

The proof of this proposition was more necessary, as pathologists who labor without the aid of botany have recently again confounded the fruits of fungi with organic concretions. This is not to be wondered at, inasmuch as they are not provided with sufficient botanical knowledge, hence they pronounce the real fruits which they know only from illustrations or preparations, inorganic forms, just as the London Medical Society did twenty years ago. To a botanist well acquainted with fungi, such a mistake could not happen. An essential support and perhaps the most important of all for my view of cholera contagion, was afforded by my rice cultures with choleraic dejections, wherefrom it results, that, in fact, the cholera fungus produces a disease in the leaf of the rice plant, which reproduces the same micrococcus from cysts as appear in the dejections of patients sick with cholera. * * * *

PARASITOLOGICAL INVESTIGATIONS.

I.

Solution of the Problem and the Method of Inquiry.

It is already known that the assertion has been made that in the measle-fluid vegetable organisms are met with. Already some persons have asserted that the small cells found in the fluid of pock, together with the tender-jointed threads, are of a vegetable form. Nearest to solving the problem, was Dr. Bender of Camburg, on whose very ably executed investigations upon the vegetable organisms in vaccine lymph I have to report. Shortly before the discovery of the vegetable forms in sheep-pock and in vaccine lymph, by Dr. Zürn and myself, it was in a certain manner (so to speak) foretold by Dr. M. Popper, of Prague :—" that measles is a fermentative process ; that the cause of the fermentation is probably organized ; that near proximity of marsh-water favors the appearance of this disease ; that the poison from the marshes is introduced into the system through the lungs and eliminated by the skin ; and that it can cling to the body and be destroyed by heat." Gorup Besanez had compared the conditions under which albumen and gelatine furnish tyrosine and leucine, and those occurrences which take place at the formation of these combinations in the animal body, with fermentative processes ; but it was already known that

in the measle-pustule an albuminous stuff, leucine, fat and salts, particularly muriate of soda, were contained; that the blood of those sick with measles contains less sugar and urea than is usual; that in their urine may appear albumen, sulphuretted hydrogen and valerianic acid, and that the latter was from the decomposition products of leucine.

Schönbein, in the year 1865, had made the important discovery, that lymph in dilute hyperoxide of hydrogen causes the development of oxygen gas, and in the same way as platina resolves the hyperoxide into water and oxygen, as he had before shown with organized excitors of fermentation.

Coze and Veltz, by an investigation of the blood of those sick with measles and of that of living animals inoculated with measle-matter and measle-blood, have essentially supported the view of Schönbein, inasmuch as they found in their blood masses of quiet bacteria.

Finally Wertheim showed that here likewise the influence of the soil, under certain circumstances, appears as a favoring force, and that the height of the marsh-water has not less influence. This last statement, therefore, is of great importance, because from our investigations result the great influence of moisture upon the organisms coming here in question. Küchenmeister further shows, that the entrance of the pock-poison takes place through the lungs.

One of the most convincing facts, arguing the material nature of contagion, Prof. von Hessling has furnished, which I give in his own words:—"Some years since a man had the measles. According to the rules of the hospital, he was kept in a separate room, where he died. The bed and bedding were destroyed. Fourteen days after, the mason scraped and re-whitewashed the walls. The mason also sickened with the measles and died, while throughout the town there was not another case of measles."

There is, then, here, an important fact shown: that the

contagion of measles adhered to stationary bodies; in this instance to the walls of the room.

The botanical questions here to be answered are therefore the following:—

First, whether in the sheep-pock, cow-pock, and small-pox, vegetable organisms always appear; secondly, of what nature and descent are they; thirdly, where do they naturally exist, and what action do they exert upon animal matter? When these questions can be definitely answered — whence and in what manner men and animals are infected with pock-contagion — then the way and the manner of diffusion of the contagion in the animal body become subjects of inquiry, as also the production of the infection by the entrance of the microscopic organisms. This part of the labor, by far of the most practical importance, requires, if its solution should not be continued for years, rooms and capital, which a private individual, much less a professional man, cannot afford. Thence this part of the labor will be advanced very slowly, as long as Government are unwilling to furnish one hundred sheep, ten or twenty cows, and provide necessary rooms for the investigation; an imputation which probably, through the uncommonly large practical consequences of this question, can be justified. A large number of animals for inoculation, and the introduction of the contagion by other methods, is for many reasons an indispensable condition for the success of the work; then, likewise, when one would not at least disregard the essential fitness of the animal, as also that every one of these organisms coming in question belonged to some morphisms (generation and its forms), we should take advantage from each of such morphisms to experiment upon and raise the yeast-form constituting the contagion. But even the soil to be used will in no way be indifferent. The substratum can greatly influence the physiological nature of the yeast which is derived therefrom.

With a larger number of individuals for experiment, the probability of a quick result is very great; with a smaller number, much less.

The means, which for the solution of the first question necessarily constitute the botanical part of the problem, I have already made known in my work upon "Fermentation." I have now to remark upon some improvements in my apparatus and method of pursuit.

The culture and isolating apparatus which I have described and figured in my work on "Fermentation," are still used in my later investigations. The large isolating apparatus has answered, throughout, its purpose. The air-pump is to be preferred to the aspirator for various practical reasons. The aspirator requires distilled water for the renewal of the air. If we would be secure from soiling, a large quantity of distilled water is needed to keep the apparatus in order, because the water which has once been used is no longer to be regarded as pure. But it is impossible that the water in such an apparatus can be free from air and dust, so that it does not answer the purpose of the perfect isolation of the culture. I say nothing of the difficult management of the apparatus.

All these inconveniences are avoided by the use of the air-pump. One obtains by it a complete isolation, and has control of the conveyance of the air. At the conclusion of each culture, the air-pump must be perfectly cleansed, and proved before the commencement of a new culture. Culture in the isolating apparatus, besides, serves for control over the result obtained in the culture apparatus; hence it should be first opened at the conclusion of the labor: especially is it the best criterion of the exactness of the obtained results, if all the cultures agree.

At present, I usually provide the culture apparatus with a bell-glass, through whose air-tight open mouth a many times bent tube is passed and secured so as to effect a slow inter-

change of the air. The cleansing of all apparatus I have lately attended to with much greater caution. For instance, all glass apparatus, as bells, saucers, &c., after having been carefully washed with water, are rinsed in a solution of permanganate of potash, 10 grains to water ℥ vi., then washed again with water, and finally rinsed in alcohol. The corks were submerged for half an hour at least in a solution of permanganate of potash. The water used in the culture apparatus was daily disinfected by means of the same solution, so that the culture might not be vitiated by the admittance of any spores of fungi. This method of proceeding has still the greater advantage, that with a dangerous contagion, as measles, for instance, it is not likely that the pestilence can be conveyed through the air by means of the fung-element. Besides, the air of the culture-rooms was daily disinfected by chlorine. For the benefit of the operator, a gargle of dilute alcohol was daily or oftener used, and the most scrupulous attention given to personal cleanliness.*

Whoever attentively continues later investigations upon contagion, or studies the vegetable parasites on animals and man, or investigates the cause of a parasitic disease, will find that there are every where the smallest and most simple yeast-formings which I have called micrococcus, and which have more or less influence. Much more remarkable is it that in cholera, sheep-pock, kine-pock, the silk-worm disease, spleen-rot, yes, to all appearance in intermittent fever, typhus and measles, the micrococcus of a well-defined fungus (or alga) is diffused in diseased parts of the body.

Nearly two years since, I advanced (in the scientific appendage to the Leipsic newspaper) the hypothesis that all

* The chapter containing a description and figures of the culture and isolating apparatus, from Prof. Hallier's work on Fermentation, may be found in chapter xi.

contagion and miasm proceed from the micrococcus of algæ and fungi, and until now, this hypothesis has gained additional strength with each new step in the investigation. Likewise with the diseases of plants, it appears that very frequently the micrococcus is the penetrating and decomposing principle, and is the contagion. Especially were all decomposing rots, as the potato-rot, the turnip-rot, according to M. Willkomm, the rot of living and dead wood, introduced by means of the micrococcus of a certain fungus. Shall the micrococcus, when derived in great numbers from the human body, be wholly inert? Such a conclusion would have something very contradictory in it; much more is the supposition enforced by the mode of action of the yeast, that this, when it appears in large quantity, likewise produces a mighty change in the tissues and secretions of the body. Thus should one adopt the opinion that these micrococcus cells, always belonging to a certain fungus, and appearing in certain diseases and in certain organs in great number, are wholly adventitious and of no importance for the causation of the disease, he would do most atrocious violence to nature and sound reason.

Thus it was preliminary for all the following labors upon contagion, as the leading maxim, the postulate must come in practice, to find the micrococcus and to raise from it the fungus to which it belongs.

Perhaps algæ furnish contagion or probably miasm; this appears more probable since Cohn has shown that the genuine vibrios belong to the Oscillatoriæ, and that they contain phykochrome. What I earlier advanced upon bare conjecture, this has Cohn with remarkable acuteness and through exact inquiry shown—that with algæ as with fungi, an independent self-development of plasma-nuclei, that is, a micrococcus-forming takes place, which I have for a year observed, and which is evident from the labors of Cohn and

Itzigsohn. Thus the algæ have yeast-forming analogous to the fungi, and it is possible that these may become destructive to human beings.

In whatever manner one may reflect upon contagion and miasm, this is certain, that if contagion and miasm are of vegetable nature, they can be nothing else than micrococcus. The evidence thereupon lies simply herein, that the micrococcus is the only vegetable structure which can pass through the most delicate capillaries. Even the smallest spores of algæ and fungi are much too large thereto; but from my admeasurement of the micrococcus of various fungi it is evident, that these small bodies of some fungi have a diameter much less than that of the most delicate vessels.

But I have further shown, that the micrococcus of certain fungi can be taken in through the lungs, can advance to the mammæ, while earlier I have shown that they occasionally appear in the blood of beasts and likewise of man.

This agrees throughout with Küchenmeister's statement, that the measle-poison is taken into the system through the lungs and can be thrown out by the skin.

I cannot close this paragraph without stating, that in the first half of this century clear-sighted men have again and again maintained the vegetable nature of contagion, more especially Prof. Frank X. von Gietl; while Dr. Cowdell in the autumn of 1849 first advanced the idea, that the cholera was produced by means of a small microscopic fungus, an opinion which was in the same year confirmed through direct observation by Budd, Swayne and Brittan. Dr. Gietl, from the year 1831, had repeatedly maintained the organic nature of cholera and typhus, and invited the microscopists to examine the dust and walls of the rooms where the sick were confined:—that infection was conveyed by material bodies, the observations upon which were to be made from all points

converging to this opinion. But are the diseases producing decomposition yeast-forming processes—they can only be introduced by means of micrococcus.

As all my studies upon vegetables appearing in contagious diseases are based upon my yeast-doctrine, it is evident, that these investigations as well as the earlier, are wholly unintelligible without an accurate knowledge of my writings upon vegetable parasites and yeast-formation. Whoever has not my yeast doctrine in mind, must take in hand both works and appropriate their contents. Whoever omits to do this, is not to be pitied for inability to understand me, or for lack of knowledge.

In reference to the nomenclature of yeast-forming some modifications have been recently introduced, of which I will give the most important, because heretofore they have appeared only in the journals.

First, I have designated, conformably to the proposition of the Professors Richter of Dresden, all those forms belonging to one fungus-species as morphisms. Generation I call the chief form, which is distinguished from the other generations by essentially varying spore-forming. Thus, for example, are Mucor racemosus Fres. and Penicillium crustaceum Fres. different generations; because the Mucor developes theca-spores, but the Penicillium acro-spores. But the varying forms in which the Penicillium appears, as for example, on the rice, Cladospore-like, as common mould in the form of normal Penicillium, &c., are only morphisms, not generations, because the spore-forming is here the same. So is it with the various forms of yeast, the micrococcus, cryptococcus and arthrococcus; further, their transition form into aërophytic forms, as for example, the Mycothrix chains (Leptothrix auct.), whose remnants were so often called Bacteria, Hormiscium, Oidium lactis, the Torula aceti, &c. are regarded altogether as morphisms.

The Leptothrix chains, that is to say, the micrococcus, which under the influence of the air on the surface of a liquid remains connected in chains, I have, according to Itzigsohn's suggestion, called Mycothrix. I have been often reproached concerning the name Leptothrix, but very improperly; for first, I was not the originator of the new name in this sense, inasmuch as Remak and others had introduced it: secondly, I do not call it, as Algologists do, a species, but a morphism, which appears in very many, perhaps in all fungi. As a specific name Leptothrix must probably be dropped, because this Alga-species is mixed up with the formations of various Algæ, not independent formations of different Algæ. But it is proper to give to the associated chain-forming micrococcus cells the name Mycothrix, to the analogous formations of an Alga the name Leptothrix. The genuine vibrios are plainly Oscillariæ, and the whole group of these consist of the yeast-like morphisms of the higher Algæ.

The mystery of the discovery of vegetable contagion consists herein, that one can raise from the micrococcus, the only vegetable form in the diseases of beasts and men produced by contagion and miasm, the higher generations and morphisms of the fungi.

This culture succeeds in a two-fold manner. Have we a strong soil whereon a certain fungus thrives, the micrococcus cells sprout in very delicate threads, which combine and strengthen themselves through multiplied anastomosis and soon develop fruit-hyphens. This furze we can call Mycothrix furze. It consists in a certain sense of very delicate Sclerotium formations. If the soil is very moist or liquid, these Mycothrix-chains have no fructifying furze, but every micrococcus cell swells slowly under the influence of a small draught of air, and sprouts, after it has increased from 10—20 diameters. These sprouting cells are of the form and sig-

nification of spores, and certainly of acro-spores; I hence name them Sporoids.

II.

Vegetable Organisms in Sheep-Pock.

Early in October, 1867, we found, under a magnifying power of 500 diameters, in the sheep-pock matter taken, in small glass tubes and hermetically sealed, from various individuals who had the epidemic, whirring cells, clustering swarms, which at once were recognized as swarming micrococcus like those of fungi. Fig. 4 shows them as seen under the highest power of one of Zeis's microscopes. They were of a dark-brown color. I at once undertook their culture.

First Culture. On sweetened water I put some of the sheep-pock matter from the glass tube, and exposed it in a culture apparatus to the room-temperature of Oct. 11, 1867. After some days there were found on the surface of the liquid micrococcus cells from two to six jointed, so that small Mycothrix chains, known as the Bacteria of the earlier nomenclature, were formed, as shown in Fig. 6. Numerous swarms of micrococcus cells were found at rest on the day after the sowing (about twenty hours after); many of them had formed chains. In each link of the chain I saw very distinctly a dark nucleus (Fig. 6.), probably for a new swarm. Such chains with a nucleus in each joint I found at different times in the lymph.

On the 16th of October, i. e. in six days, there were to be seen at the boundary of the liquid from swelling micrococcus, heaps of larger olive-colored, round, and at last lancet-shaped cells with one or more nuclei (Fig. 7), which on the following day had become larger and partly sprouted (Fig. 7), so as to exhibit sporoids. While these sporoids

were sprouting at the brink of the liquid, they had developed micrococcus under the surface of the liquid, so that on the 14th day after the sowing I found in the liquid numerous burst spores (Fig. 8), by the side of which lay a heap of quiescent micrococcus and other spores not yet empty, in whose plasm numerous cells appeared, instead of the original central nucleus (see Fig. 8).

At the brink of the fluid in the vessel sprouting sporoids were seen; at the end of each irregularly divided sprout were chains of spores (Fig. 9), at first blunt and broad lanceolate, at last globular. These sprouting Monilia-like bodies were at first pale or colorless, at last the spore became olive-colored, dark-brown, compact, like a Cladospore (Fig. 10), and the more so, as the lower links of the chain were becoming narrower and longer, like Monilia spores (in Fig. 10), and at last divided by septa (*c l*, Fig. 10). Some of the Monilia spores became very large, of a pale-brown color (*m*, Fig. 10), and closely resembled the well known Monilia cinerea of Bonorden. The developed Cladospore is not to be distinguished from Cladosporium herbarum auct.; however, I should hesitate to say that it was the shape or form which would enable me to identify it in the absence of other facts for its identification. Before many weeks had passed, upon the inner surface of the cork, by whose means the downward-bent glass tube was fastened into the receiver, a frail vegetation of Penicillium crust. Fr. was plainly seen, while at the same time in the liquid appeared sterile threads of the same fungus. Further off was found in the liquid, particularly at the bottom of the vessel, the mycelium of Cladospore, not a chain of spores, but single, large, yellowish spores not as yet forming sprouts, but mostly Tilletia spores forming micrococcus (see Fig. 14).

This culture was continued up to January, 1868, or for three or more months, and without the appearance of any other than the anticipated forms.

SECOND CULTURE. From a tube containing the lymph of sheep-pock a portion was blown upon the white of an egg, and placed in the culture-apparatus at the room-temperature of Oct. 18, 1867.

There was seen, on the day after, a change like that in the first culture, to wit, Mycothrix forms of the micrococcus cells which had come to rest, then appeared at the brink of the liquid in the vessel, later in the midst of them, a crop of sporoids (Fig. 12) out of the slowly swelling micrococcus. These sporoids were of a brownish color, and sprouted as soon as they had attained their full size (Fig. 12). Already early in November among the sporoids heaps of sprouts appeared, a part of which fructified in the form of very fine and strong specimens of Cladosporium herbarum Link. (Fig. 11) ; at the brink of the liquid, also, from which the fungus, by and by, spread out on the wall of the vessel. Amid the white of egg, at least close under the surface, the sprouts from the sporoids extended, but they formed large, nucleated, short, and at last globular joints and yellow spores (see Figs. 13,25), which were single or connected in small chains at the ends of the branches (Fig. 13), forming by degrees a cross-barred or reticulated epispore, and by this means was formed a Tilletia-spore (Fig. 13). These cross-barred or rèticulated spores are different from those of Tilletia caries Tul., in particular by their golden yellow color.

I would attach but little importance to the size. As we shall hereafter see, they belonged probably to Tilletia lolii Tul. These spores do not sprout on the white of egg, but they form micrococcus, which they leave after the irregular bursting of the epispore (Fig. 14). The micrococcus formation is essentially different from that of Tilletia caries Tul., but analogous. Out of the sprouts or branches of the sporoids come Cladospore-growths, which advance higher along the surface of the vessel and very soon produce, be-

side the Cladospore-chains, the Sporidesmus-fruits which are sometimes single, sometimes in chains, already known through Tulasne. Whoever compares the figures with those of Tulasne will have no reason to question their identity. In this opinion they will be confirmed by those communications which I shall hereafter give.

The culture-apparatus remained unopened for several months. The consequence was, that the varied formed fruit of Sporidesmium had dissolved their walls and had formed masses of black micrococcus. These were in such masses and so dark that the wall of the culture-apparatus had become very much obscured.

The following is in short the result of the second culture. From the micrococcus of sheep-pock sporoids formed, from whose sprouts came, in the air, Cladosporium-Sporidesmium plants, which appertained to Pleosporium herb. Tul. Within the substratum form male sprouts out of large globular joints, which, as is the case with Ustilago carbo. Tul., are formed by the division of lyrate cells, and which probably belong to Tilletia lolii Tul.

Third Culture. On paste moistened with a solution of tartrate of ammonia, sheep-pock matter was blown from a tube and exposed in the culture-apparatus to the room temperature of Oct. 12, 1867.

On the first day after the planting, micrococcus, after resting, were extraordinarily multiplied, and formed a brownish layer on the paste. At the brink of the liquid in the culture-apparatus sporoids were to be seen, which brought forth Sporidesmium-plants, exactly as in the white-of-egg culture. Arising from the midst of the paste were single sprouts from sporoids with Tilletia fruit, but fewer in number than in the white-of-egg culture. These brought forth a Thecaspore-fungus, which could not be very closely defined, but reminded one of imperfectly fruited Rhizopus

nigricans Ehr. That this imperfectly developed fungús is really no other than imperfectly developed Rhizopus, the following culture demonstrates. The fungus commenced, as seen, from the sprouts of Tilletia, which were often covered with very tender epispores very similar to those of Mucor racemosus Fres., having the appearance of Oidium (see Figs. 27, 32), when the spores, or more truly Macroconidia, appeared in pale chains. But the Macroconidia of the Mucor racemosus are at first quadrilateral, oblong, broad-ovate, and at last globular. They were in the above named culture, as of Rhizopus, broad-oblong and blunt lancet-shaped, at last globular (Fig. 32).

At the brink of the liquid in the culture-vessel appeared, also, among the sporoids from Cladosporium, Penicillium crust. Fr. of the usual form. It is very probable that some of the micrococcus cells contained in the lymph or serum belonged to Penicillium; and, as I have shown, that now and then in the blood of healthy men and animals, as in milk and colostrum, micrococcus-cells belonging to Penicillium appear, so this might have happened in the sheep-pock and be nothing strange. These only accidental and exceptional micrococcus-cells of Penicillium, have in all probability no connection with the cause of this disease in the animals.

After the culture had been continued a week longer, there appeared at the boundary or edge of the paste, numerous purely vegetative mycelium-threads of Cladospore, which had long, continuous joints (Fig. 22); each of these joints had a long row of shining nuclei.

Upon the dryer surface of the vessel were to be seen the same threads; here, however, very slender (Figs. 23–24), of a pale purple color, and with strangely coiling branches (Fig. 23). At the spot, where such twisted branches terminated, was found a large, many-celled ball, at first reddish,

but later of a brown color. Thus there appeared to be here the forming of fruit, perhaps a genuine fructification. The final product, the result of the year's culture, unfortunately was not obtained, which is to be regretted, as nothing certain is known of the impregnation of Pleospore. It is possible that here is presented the first unfolding of Pycniden or Peridien. This last view, viz., that here the first condition of the real Pleospore-fruit is formed, may be regarded as very evident.

By a more excellent method of the remainder of this culture, the connection between Cladosporium and Tilletia was established; for at the edge of the substratum the genuine Cladospore-spores came out from one and the same mycelium-thread (Fig. 25), beside lyrate cells (*l*, Fig. 25), out of which the Macroconidia (*m*, Fig. 25), and further off within the substratum Macrospores (Tilletia spores) were found. High above the paste arose out of the micrococcus-cell, on the entirely dry surface of the vessel, no sporoids, but a crop of Mycothrix furze, from which very delicate Cladospore-plants sprung up.

Fourth Culture. — On a slice of a peeled lemon was placed, on the 11th October, 1867, a quill charged with sheep-pock serum, which was put into a culture-apparatus. Eight days after the sowing, there was to be seen on the surface stout specimens of Rhizopus nigricans Ehrenb. (Fig. 28), and on the whole of the furze were arranged groups of a brownish-colored vegetation of Cladosporium herbarum. On the more moist parts of the fruit, there were at some places much larger spores of Cladosporium, which, as in the first culture, took the form of Monilia-spores, and indeed of the spores of Monilia cinerea Bon. (*m*, Fig. 27); in this manner they were passing over to Macroconidia. In the following week, some specimens of Penicillium crustaceum Fr. came to light, whose growth at first covered the cut-surface

and overrun the two other fungi. The culture was now set aside. For the rest, there appeared beside the Penicillium, in particular on the surface of the slice, still other Penicillium with colorless spores which I call Penicillium grande, an account of which will be found hereafter.

Fifth Culture. — A small portion of sheep-pock matter, in particular from the gland-follicles, which was abundantly supplied with micrococcus, was, on the 20th of October, put into a culture-apparatus on paste prepared with a solution of tartrate of ammonia.

On the somewhat thin part of the paste Cladosporium was developed as in culture No. 3, besides, Oidium lactis and Arthrococcus lactis. On opening the apparatus, the substratum had a very acid reaction.

Sixth Culture. — On the same paste was put in a larger isolating apparatus (for the purpose of confirmation) some of the sheep-pock matter, and the apparatus left unopened until the 25th of December, during which time fresh filtered air was daily supplied by the air-pump. On opening the apparatus the result was the same as in the preceding culture.

Seventh Culture. — Starch paste, with some white of egg and a bit of sheep-pock matter, was put into a culture-apparatus on the 20th of October. The result, after fourteen days, was the same as in the preceding culture, with this difference, that some plants of Penicillium crustaceum Fr. of a normal development were obtained, in consequence probably of the more dry soil.

Eighth Culture. — On the first of November, on a slice of peeled pear, was put a quill charged with sheep-pock matter, and placed in the culture-apparatus. Fourteen days after, the cut surface of the pear was covered with a vegetation of Monilia cinerea Bon. (Figs. 27, 32), whose spores (Macroconidia) put forth vigorous specimens of Rhizo-

pus nigricans (Fig. 28). This culture was repeated because of its importance, and with exactly the same result.

Ninth Culture. — Some of the Cladosporium-sporidesmium-plants from culture No. 2 on the sliced pear, were put into the culture-apparatus. Fourteen days after the sowing the pear was covered with Rhizopus nigricans.

Results of these Cultures.

From these nine cultures, made with the matter from various sheep affected with the epidemic, it appears to be clearly proved —

1. That among sheep-pock matter always appeared the micrococcus of Pleospora herbarum Tul.

2. That Pleospora stands in the same relation with Rhizopus nigricans, and with a Tilletia, Tilletia lolii Tul.

The questions now to be answered are—Whence came the Pleospora herbarum? and how were the sheep infected with this fungus?

The answer to the first question is very indefinite. Pleospora herbarum Tul. comes as a disease (Russ-brand) on the green parts of many plants; further, as I might show, on the wood of the vine, on the dry spots of the peel of the apple, pear and plum, and in particular of the lemon. I examined the fruit of the Pleospora and found it to be closely comformable with Tulasne's description. A more perfect knowledge of the generations and morphisms is desirable; for which I undertook the following cultures.

Tenth Culture. — On the first day of November, 1867, I sowed on the cut surface of a peeled pear some spores of Cladosporium herbarum Lk. from the peel of another pear. The sprouts were at the commencement like Cladosporium (Fig. 32), yet most of the spores were larger, broad, and blunt-lanceolate, toward the end of the chain globular

and often very large (Fig. 32, *m*), sometimes divided like Puccinia-spores (*p*, Fig. 32). The large spores (Macroconidia) produced vigorous specimens of Rhizopus nigricans Ehr. in eight days after sowing. Likewise the pale lanceolate Monilia spores sprouted, and brought forth Penicillium grande (*m*, Figs. 30, 33), which I have alluded to, with pale, large acrospores and oppositely branched pencils. This Penicillium, in a certain manner the unripe fruit of Botrytis (Fig. 36), sometimes resembled vigorous specimens of Penicillium crustaceum Fr., from which they are discriminated by the more opposite ramifications of the pencils, by the coarse-nucleated and shining plasma in the hyphen, and by the spores with the shining nuclei, and their distance from the including membrane. I did not see that this perhaps too rich culture produced any specimen of Botrytis.

Eleventh Culture.—On the third of November, 1867, Monilia cinerea Bon. was sown on the juicy peel of a sliced apple. On the ninth day after the sowing, a crowd of Cladosporium herbarium Lk. was found on the peel. On the cut-surface small specimens of Rhizopus nigricans Ehr. appeared. This Rhizopus arose (as more exact inquiry shows) from the sprouts of Macroconidia, i. e., from unripe Tilletia spores, which were not on the surface, but within the fruit-pulp. It is therefore here, as with Penicillium-Tilletia-Mucor, the Mucor-fruit (Rhizopus), a product of anaërophytic generation (Tilletia), either in a ripe state (Macrospore or Tilletia), or in an unripe condition (Macroconidia).

Twelfth Culture. — On the third of November, 1867, on the cut surface of a succulent pear, Monilia cinerea Bon. taken from a plum was sown. The Monilia grew and multiplied rapidly; in a few days were seen the sprouts in the form of Penicillium grande (Figs. 32, 33), which at last became brownish, and took closely the form, mode of branching and fructification of Botrytis elegans Corda (Fig. 36).

The Botrytis is therefore a true acrospore-plant. It is seen very frequently in company with Rhizopus, and it stands to Rhizopus in a similar relation as Sporodinia to the Syzgites of Schacht, or rather as Penicillium crust Fr. to Mucor racemosus Fres.

I repeated the preceding culture many times, not always with success; frequently on too moist soil, the fungus appeared only in the unripe form of Penicillium grande. This fact was confirmed by every culture — that Rhizopus never appears on a surface which is exposed to the air, but that its first mycelium, as the product of Macroconidia-sprouts, issues forth from the inner part of the fruit-pulp; hence it was not often at the place of the sowing, but at some distance from it.

Thirteenth Culture. — On the seventh of November, 1867, the above-mentioned Botrytis was conveyed from the peel of a pear to a peeled slice of another pear. On the fourth day after came forth from the sprouts Monilia cinerea Bon., in beautiful specimens. From this culture, however, no fruit pencil developed. We find Botrytis on the peel of fruit, but only in a particular moist condition, which is with difficulty arranged and kept for any length of time. The yeast-formation, which is with so much difficulty avoided, increased the amount of moisture in the soil, and by this means the culture was interrupted.

On several spots on the pear Rhizopus appeared, and the same thing happened in another culture.

Fourteenth Culture. — Rhizopus nigricans Ehr. was sown on paste moistened with tartrate of ammonia; the sprouts of Rhizopus (Fig. 34) formed interstitial Macroconidia, like those of Mucor racemosus Fres., varying in color from a yellowish-red up to a rose-red, and mostly larger. I did not again succeed in raising, from the Rhizopus, Botrytis or Pleospora, because I did not succeed in forming the

necessary condition. Rhizopus was reproduced during a longer time, as indeed is the case with Mucor, when it falls on propitious soil. I had now to examine whether Tilletia lolii has any relation to Pleospora herbarum. In October I carefully examined numerous spikes of Lolium perenne L. I found the Tilletia already scattered in dust, but strangely all those flowers most plentifully attacked with Pleospora, which the sprouting Tilletia spores showed in greater numbers on the glumes, &c. This relation should be closely examined by the sowing of Tilletia lolii Tul.; as, however, the Pleospora forms an anaërophytic spore-form, which is readily distinguishable from Tilletia lolii, as further the Tilletia is almost always to be found in those Lolium plants, which are attacked by Pleospora at all periods of the shoots, which J. Kühn has so clearly described and figured, so at least it is very probable that the Tilletia lolii Tul. was the parent of the Pleospore; and so consequently that the Lolium plants had infected the sheep with Pleospora. I now sought Pleospora herbarum Tul. on the pear-slice used for cultivation, and had the pleasure to find always, after about eight days, that Rhizopus was reared, as if after the sowing of the fruit of Pleospora.

This result corresponds remarkably with the opinion of veterinary physicians as to the way in which sheep are infected with the pock. It is the general opinion of the most distinguished physicians and breeders of animals, that spoiled hay is the cause of pock-disease. As the Pleospora is known to reproduce itself as long as it finds a suitable soil, so it can be diffused from the Lolium plants to the grasses or other green parts of plants. Here, however, in Thüringia, I am sorry to say, are Lolium-borders not only allowed but even protected to the ruin of the corn-fields.

As I have already shown, the Lolium-borders supplied the fields with (Mutterkorn) smut, but supplied the sheep with

Pleospora. If the micrococcus is identical with the contagion of sheep-pock, so that we are permitted to draw therefrom the most practical inference, it is possible to succeed in preventing this disease. To understand what has been said, an accurate knowledge of the yeast-formation of Pleospora is indispensable. We have above seen that the micrococcus of sheep-pock largely multiply in nitrogenous soil (Fig. 18), and that they form sporoids on wet, and Mycothrix fungus on dry soil. On sowing the micrococcus of sheep-pock on the succulent slice of pear, also in glycerine, from the swollen micrococcus-cell first came Cryptococcus in globular sprouting cells (Fig. 19) ; on the liquid's becoming sour, Arthrococcus, as Fig. 19 shows in a young state; in Fig. 20 it was in its ripe condition. Quite the same appearance takes place on the glumes of Lolium-grass, when it is covered with Pleospora and becomes wet. Figure 31 shows a small fragment with a vegetation of Pleospora of all possible forms. At *t*, empty Tilletia spores; at *p*, those of Pycniden lay around. As soon as the cell-texture becomes moist, the spores by degrees swell and discharge their contents as micrococcus, which always covered the glume in great numbers after moist weather (Fig. 31).

From these micrococcus we can very early raise, on cooked fruit-pulp, cryptococcus and arthrococcus. It must therefore be, that wet hay necessarily infects the atmosphere with micrococcus and Pleospore, and sheep which are exposed to moist hay provided with Pleospore, must inhale them. Thus they become diffused through the body and are thrown out by the skin.

A very important fact, in a pathological point of view, I must not omit to mention. During my labors I experienced, from the repeated opening of the apparatus, and the frequent sowings of the sheep-pock lymph, a severe bronchial catarrh with a painful cough. In the tough expectoration

I found large numbers of the brown-colored micrococcus, as they appeared in the sheep, which must have greatly multiplied on the mucous membrane. I cultivated them on paste moistened with a solution of tartrate of ammonia, and with pleasure saw within 14 days, from the sprouting micrococcus, that a vegetation of Cladosporium, &c. covered the paste, and afterwards Monilia in large numbers appeared within the paste, while Tilletia were developed.

From the entire labor it follows, therefore, with absolute certainty, that the micrococcus always appearing in sheep-pock matter belongs to Pleospora herbarum Tul. These results are of importance for the pathology of the disease. * * * *

To give all in a short resumé, the Pleospora, in the Cladosporium form, produces on soft nitrogenous soil Macroconidia and Macrospore (Tilletia), the first on more moist soil sprouts and generates Rhizopus; the second exhibits more durable spores, from which, after longer repose, Rhizopus is brought forth. On the surface there forms from the Cladospore-form, Monilia-Botrytis.

Thus we get the following parallelism:

1. Acrospores.	2. Thecaspores.	3. Anaërophytic generation.
Penicillium crust. Fr.	Mucor racemosus Fres.	Tilletia caries Tul.

4. Generation descent.	5. Arthrospores and Schizosporangia.
Achlya.	Cladosporium and cysts.
Fungus Ascophorus.	Cladosporium Sporidesmium.

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III.

Vegetable Organisms in Vaccine Lymph.

For the material of these investigations I am indebted in part to the institution for vaccination at Hamburg, but mostly to the kindness of the central vaccinator, Dr. Reiter, of Munich.

There was found in the kine-pock lymph from both places large masses of micrococcus-cells and Mycothrix chains. The micrococcus is so very small, that under a magnifying power of 1000 linear it appeared still punctiform (Fig. 4). In the lymph I found the micrococcus mostly quiescent, but sometimes swarming.

The cultures gave the following results:

First Culture. On paste made with phosphate of ammonia, I put, on the 16th Nov., 1867, the contents of a vaccine tube from the Munich institution. In the first hours after sowing an extraordinary number of micrococcus were to be seen. After twenty hours, the starch granules looked as shown at Fig. 44, *a*, *b*, *c*. The much swollen paste granules were seen with distinct cracks or fissures, which extended from the centre to the circumference. In these fissures (Fig. 44, *a*,) the micrococcus-cells were considerably multiplied, and crowding together in the less dense strata of the nucleus (Fig. 44, *b*, *c*), where they formed concentric zones.

On the following days the micrococcus-cells had swollen by degrees to sporoids, which sprouted (Fig. 45). These most delicate sprouts, at first by degrees becoming stouter, furrowed and perforated the starch body, which sieve-like, or more often carious, fell to pieces and dissolved entirely.

Meyen and Martius first intimated that the starch granules are perforated by small fungous threads; later Schacht has succeeded in proving this. From observations on the

potato-rot I succeeded in showing that the fungus, as here, attacks the starch granule by the micrococcus advancing to the kernel (Korn) and growing into sporoids.

The sprouts of the sporoids represent moderately thick, irregularly branched fungous threads, which, after about fourteen days, brought forth pencils of Penicillium crust Fr. Naturally these Penicillia are not from the micrococcus of kine-pock, but from such as had entered in with the breath and were blown through the tube. The Penicillium pencils, however, at first were not normal, but showed exactly those variations which I have previously described and figured as a bastard-form between Penicillium and Aspergillus. There is in fact the mould-covering, which settled on the sowing, of which we spoke, composed of Penicillium, Aspergillus and the bastard between them. As in all such cases, where the soil is moist, Penicillium very soon perfectly suppresses Aspergillus, so here also, the Penicillium suppressed the Aspergillus and the bastards in a short time.

Bail regards them only as the transition from Penicillium to Aspergillus. I have not surmised, but noticed this fact—that here a bastard production takes place. When it is seen that a typical specimen of Penicillium and a typical specimen of Aspergillus fuse, and the hyphens, sprouting from the united specimens, are seen bearing that kind of pencil that we can regard them as neither Penicillium or Aspergillus, then it is proved, that here is a bastard formation, not a transition. The transition of one fungus species into another has not as yet been observed by any one. From Ustilago carbo Tul., Aspergillus can easily be raised, when a proper soil is selected, as it is easy, on a proper soil, for Mucor racemosus and Penicillium crust. Fr. to be reared from Tilletia caries Tul. But from no soil can one raise Penicillium from Ustilago, or from Tilletia an Aspergillus, so long as the culture remains pure. But as

soon as either fungus simultaneously appears, there is formed at the points of contact a fusion, and now come to light, immediately, bastard-pencils. Nowhere will one sooner be convinced of the exact limit of plant species than in the lower groups of fungi. To get, if possible, in the same culture other generations or morphisms, I put under the culture-apparatus a cork, because Aspergillus, as it is well known, loves a dry soil. The cork was well prepared, as in all similar cases, in the following manner: it was kept for half an hour in a solution of permanganate of potash; afterwards, for the same length of time, in alcohol, before being used in the culture. After about fourteen days there was to be seen on it a tender mould, which under the lens proved to be the smallest Mucor I had ever seen, by the naked eye scarcely to be seen at all. It is represented in Fig. 46, T. 1, as seen by the aid of a strong lens. The fruit bearers are mostly dichotomous, seldom single.

An Oidium fruit precedes Mucor, which could with difficulty be distinguished from Torula rufescens Fres. To this identity or diversity I shall return, and will here only remark, that in my earlier careful investigations of Torula I was reminded of the constant association of these fungi with Aspergillus. I have said already that the Torula was an Oidium; that Oidium belonging to Mucor shows at first small, globular, pale, reddish-brown conidia, very like those of Torula rufescens; these later became larger (Macroconidia), appearing as well interstitially as at the termination (Figs. 47, 48, T. 1, and Fig. 1, T. 2). I am not able to distinguish the interstitial conidia of the Mucors, which, as far as I know, was first done by Bail, from the terminating Macroconidia, which brings about the intermediate forming of Acrospore generations (Penicillium, Aspergillus, Botrytis), into the Thecaspore generations (Mucor

racemosus, Mucor mucedo, Rhizopus). In all three cases there come forth from the germination of the interstitial Macroconidia, as well as from the terminating Macroconidia, Thecaspore-plants.

Bail observed the interstitial Macroconidia which he calls Gonidia, while others called them Gemmen in Mucor racemosus. With Rhizopus they are not less abundant and are mostly embedded in the soil. The Macroconidia of our Mucor on the cork form slender threads without septa, single or in chains, showing a highly remarkable peculiarity, viz., that of a Plasmodium to melt together when it finds others like itself. It is very tender, without a plain membrane, filled up with granular plasma, at least at spots (T. 2, Fig.1, pl.). These form at such places larger or smaller swellings. These swellings are especially where two or more branches of shining threads melt together and connect laterally in Oidium-chains (Macroconidia-chains), which come to development in the thread. These lay together and are irregularly folded (T. 2, Fig. 1, *v*), in a slimy sac rich in plasma.

The fruit-bearers sprout from the cast-off as well as from the Macroconidia connected with the threads, as also from the above mentioned intumescence of the threads rich in plasma. The fruit is globular (T. 1, Fig. 48). Sometimes I saw in the bearer, enclosed Macroconidia (T. 1, Fig. 48, *m*). In the ripe fruit one does not see the basal cell (Columella), but after the casting off of the small, globular, colorless spores (T. 1, Fig. 48, *sp*), the basal cell appears as a round bladder (T. 1, Fig. 49), separated from its bearer by a septum, and on which some spores still are seen (T. 1, Fig. 49, *sp*).

I saw the tender thread of the Macroconidian-plant springing out of a very mis-shapen Sterigma of a bastard Aspergillus (T. 1, Fig. 47). The fruit-bearer, before the ripening of

the spores, is filled with plasma (T. 1, Fig. 48). After discharging them it is always empty (T. 1, Fig. 49), wherefrom it follows, that here also, the Columella, as with Rhizopus, comes to development as a real basal cell, at the time of the ripening of the spores. After discharging them the wall of the bearer shows plainly a striated appearance (T. 1, Fig. 49).

Second Culture. The contents of a vaccine tube from the institution at Munich, was put on a slice of peeled lemon and placed in the culture-apparatus on the 15th of November, 1867. Four days after the micrococcus had appeared in large numbers on the cut surface of the slice. Small sporoids on the dry part of the substratum (T. 1, Fig. 50) were sprouting (T. 1, Fig. 50, *k*), and at spots had already brought forth long threads, and surely bastards between Aspergillus and Penicillium. The Penicillium had increased so much at the end of the week that I thought it best to leave the culture and commence another.

Only on the white furze of the fruit, at some spots, Aspergillus on the first days remained nearly pure and exhibited, as always under such circumstances, the form of Oidium albicans Auct., i. e. Soor, or apthæ-fungi. The form, as I have repeatedly shown, is no other than the unripe Conidien-form of a Cladospore belonging to Aspergillus-Ustilago. Here also in the lemon was to be seen very early ripe Cladospore-spores, at first long, fusiform, divided by a septum, then shorter, one-celled and at last globular. I have so fully described and figured these Cladospores in my work on "Parasites," that I would for convenience refer to that work, being content to delineate in Fig. 52, T. 1, some spores with their characteristic appendices. As of the greatest importance, I must not omit mentioning, that for some time Dr. Bender, of Camburg, experimented with, and raised from vaccine-lymph the Oidium albicans Auct. Thus he is the

first who discovered that in the lymph of kine-pock vegetable organisms constantly appear. When now, two inquirers, independently, and at different times and with different material, i. e. from different sources, experiment and come to the same conclusion, so is the security in the correctness of their judgment which but few labors can possess.

The threads of Oidio-Cladosporium crowded together deeper in the lemon; then there were to be seen Ustilago-spores, which never ripened, but were changed into large, globular Macroconidia (T. 1, Fig. 51). These strongly resemble what, in the above work concerning Sclerotium, I saw coming forth at the ends in the specimens of Torula rufescens Fr., and which I compared with the spores of a Peronospora. I called them "Macroconidien" on account of their resemblance to those forms from Mucor, and indeed, one would certainly, on comparing my Figs. 14, 15, 16, in the above work, conclude that they are no other than the Macroconidia of a Mucor.

On the sliced lemon had come forth those Macroconidia and perhaps a Mucor, which resembled Mucor mucedo, but it was somewhat stouter than that in the First culture.

Third Culture. Upon the white of an egg, on the third of November, 1867, the contents of a kine-pock tube from the Institution in Hamburg was placed in the culture-apparatus.

The micrococcus multiplied abundantly and put the albumen, as it were, into a violent fermentation. There were early developed very perfect specimens of Oidium albicans (Tab. 2, Fig. 2), whose spores ripened into Cladosporium (*cl*). The older spores, or more exactly, the short globular joints of the thread, had the appearance of Ustilago-spores, which immediately grew luxuriantly, as is the case in the culture with Ustilago.

Fermentation hindered the further development of

these Cladospores or any other morphisms. The culture was set aside for more than a month. Upon the coming of Cladosporium from micrococcus I have something more to add—that the forming of sporoids and their sprouting (T. 1, Fig. 53, *k*) is plainly to be followed.

Remarkable, and for a correct judgment of the origin of kine-pock, this fact is of the greatest importance, viz., that the white of the egg is colored of a beautiful wine-red by the reddish-brown micrococcus; they are somewhat lighter colored than the spores of Torula rufescens Fr.

Fourth Culture. As I knew that the Aspergillus loved a dry soil, I put, on the 15th January, 1868, some kine-pock lymph from Munich, on a cork disinfected by the permanganate of potash and alcohol. On the 22d, there was to be seen on the cork, in the apparatus, a gorgeous typical specimen of Aspergillus glaucus Lk (T. 2, Fig. 3). The spore-heads were snow-white and became greenish on the following days. The white spores, when dry, showed their well known spicular surface (T. 2, Fig. 3, *s p*); in alcohol they seemed smooth and presented a large shining nucleus (T. 2, Fig. 3, *a s p)*. The fruit-bearer proceeds perpendicularly from the tender empty threads, and these mostly formed at their connection a horse-shoe shaped foot or root (T. 2, fig. 3, *s*). The bearing threads are here and there septate, but only at more delicate prolongations, mostly pretty far from the pencil-bearer. This stands in open communication with a part of the thread. Hence, Corda, when he described the bearer as much divided, must have had another plant in view. On the 6th of February, there was to be seen on the cork the first bright specimen of Eurotium herbariorum, which on the following day had developed to a larger mass. * * * *

Fifth Culture. On the 6th of January, 1868, the contents of a vaccine-tube from Munich was put on milk

which had been boiled an hour. On the 20th of January, the cork bore a vegetation of Aspergillus glaucus Lk. and of bastards between Aspergillus and Penicillium (T. 1, Fig. 54). On the surface of the fluid appeared a rich Arthrococcus vegetation, on which we can easily study the development of Arthrococcus lactis from the micrococcus (T. 1, Fig. 55), and observe it in all its intermediate stages.

About four weeks after the sowing on the cork, began the fruitage of tender Aspergillus threads, and the development of brilliant typical specimens of Eurotium herbariorum.

Sixth Culture. On the 23d of January, 1868, a small number of micrococcus on the white of an egg (from Culture No. 3) was placed on a disinfected cork, which lay in a porcelain vessel in the culture-apparatus.

Fourteen days after the apparatus was opened, micrococcus cells in all stages of sporoid-forming and sprouting were to be seen. The sporoids sprouted to beautiful typical specimens of Torula rufescens Fr. In this fully developed condition this fungus is no other than Botrytis Jonesii, from which already Itzigsohn and De Bary have shown, that it is a conidien-form of Mucor mucedo Fres. The spores, at the commencement, are broad-lanceolate, brownish-red, and arranged irregularly in chains. The mycelium is creeping.

Thus the plant here furnished a typical Torula rufescens, as one may find figured by myself in the Botanical paper (1866, T. vii.). Later, the hyphens raise and bear panicularly-arranged branches, with globular, mostly pale, but often fox-red spores, which Itzigsohn, in an autograph letter, has described. This is the Botrytis Jonesii of authors. The barren branch-ends, which one occasionally finds described and figured, probably rest upon an erroneous observation. I always found, upon careful management of my preparations, the branches covered with conidia, but they fall off easily, and thus are found some single barren

terminations. This same Torula bears, beside small conidia, much larger, globular Macroconidia. These come, as seen, with the conidia of Botrytis, and often at the same time and on the same thread with Sporangiola (Ascophora elegans, or Thamnidium), of Itzigsohn and De Bary; yet there is often seen merely large, single, globular Macroconidea at the end of the branches.

The Macroconidea sprouted and developed beautiful specimens of Mucor mucedo Fres. This fungus is wholly different in its form as well as its mode of living from Mucor racemosus.

While the Macroconidea of Mucor racemosus Fres. loves a wet nitrogenous soil, the Mucor mucedo Fres. developes on nearly dry soil. One gets on milk Torula rufescens Fr., but only on the perfectly dry milk, hence the cultivation requires some months. Further, the Torula and its highest form of development, the Botrytis Jonesii, love darkness, as Dr. Itzigsohn wrote me, and I find his opinion confirmed throughout. Aspergillus and Eurotium also thrive best in the dark. These facts are of the greatest interest in connection with the knowledge obtained in England, that smallpox heals in the dark without leaving scars. Mr. Bulmerincq agrees with me in the matter of fact, that sunlight in a short time renders the kine-pock lymph inefficacious. No Mucor is so variable as regards the size of the spores and sporangia as Mucor mucedo Fres. The largest specimens, from almost dry nitrogenous soil, equal in size the spores of Rhizopus nigricans Ehr. They show, also, a more tender form of mycelium, so that one might, without the lens, take Mucor mucedo in this form for Rhizopus nigricans, when he does not consider the kind of ramifications and the somewhat transparent, never wholly black and opaque condition of the sporangia.

As Itzigsohn and De Bary have correctly delineated, the

smaller sporangiola (Thamnidium), which are crowded on richly ramified branches, are one-celled, while the larger and even the smaller sporangia which are standing single at the ends of the longer branches possess, at least after the strewing of the spores, mostly a clavate protruding basal cell (Columella). Itzigsohn has described in his manuscript notice the beginning of the Columella as follows, which I can confirm.

"Through the hyphen a canal goes to the young sporangium, which is visible only when dry. Through this canal the plasma is always running to the young sporangium, for the forming of the spore-mass. When sufficient plasma is carried to the sporangium and thereby its figure becomes round, then the point of the hyphen becomes closed near the lumen of the sporangium by means of a vaulted wall, and by this means the Columella appears, which hence is seen only in the larger sporangium, while it is wanting in the smaller."

Therefore this occurrence is strictly the same, as I have already described concerning Rhizopus, only that with Mucor mucedo Fres. the basal membrane is in general earlier formed. The Mucor mucedo Fres. has sometimes, Rhizopus perhaps never, a proper basal cell, while I found this to be the case more frequently with Mucor racemosus. The genuine Mucor mucedo is readily to be known by the color and form of the spores. These, to wit, are never, as is always the case with Mucor racemosus Fres., truly globular, and colorless when seen single, but always oval, stretched out in length, and in a ripe condition always of a slate-grey or violet color. But these observations Itzigsohn has already made, whose Mucor studies I regret are not as yet published. Sometimes the color of the spores is of a splendid violet.

The color of the sporangia to the naked eye, as also under

the lens, is from a light grey-brown to a blackish-brown, always with an admixture of violet and often of a pure violet. The sporangium membrane, as some say, subsequently decays into a small granule, which neither Itzigsohn or myself have as yet noticed. Oftener, the sporangium-wall, irregularly torn in pieces, remains in the liquid; it may be torn off near the base, or the pieces still remain in connection with the base of the columella.

The ramification of the hyphen is very different. From the corymbose, richly ramified hyphen of the Ascophora elegans (Sporangiolen-form) up to the wholly branchless single Mucor-head, are to be seen all possible gradations, which depend on the quality and degree of moisture of the soil. On liquid soil, there form only sporangiola and very rich cymose and loosely ramified sporangia on tender web-like and prostrate threads; on the other hand, on strong and nearly dry soil, a single or slightly ramified hyphen, some millimetres high, appears.

The hyphen possesses one characteristic which never fails, viz., the appearance of transverse membranes. Mucor racemosus has generally only a few of these transverse septa in the mycelium-thread; never are they found in the hyphen-branch bearing a sporangium. Never in Rhizopus, does the transverse membrane appear in the fruit-bearing thread, very rarely in the mycelium. The fruit-bearing thread of Mucor mucedo Fres. very seldom is devoid of the membranes or septa, and for the most part these are very rich, as Fresenius and De Bary have already figured.

The Macroconidia of Mucor racemosus Fres., as I have often shown, are nearly always globular; those of Mucor mucedo Fres. are generally oblong, often fusiform. The interstitial macroconidia (Gemmen or Gonidia) are always quadrangular, as Itzigsohn has observed. They are mostly very numerous. No species of Mucor wants these forma-

tions, which have for every one a characteristic form. That they do not germinate is a mistake. They sprout very readily and bring forth new sporangia.

Eighth Culture. On the 27th of January, 1868, a small portion of the micrococcus from the sowing on the white of egg (No. 3) was put into the culture-apparatus on a sliced lemon. After 14 days, on the furze of the lemon Cladosporium (Oidium albicans auct.) appeared, among which, spores like those of Ustilago had formed confused chains. From these blackish chains the curious fruit, which I have figured (in T. 2, Fig. 7), appeared here and there, and which I maintain to be Pycniden of Eurotium herbariorum. Thus there are here, first of all, large globular-cells (T. 2, Fig. 7, *u u*), which are much divided by septa: frequently they are attached on long-celled threads, and are by these septa, Sporidesmium or Stemphylium-fruit (T. 2, Fig. 7, *s p t*). Sometimes there are in these cells kidney-shaped sporidia, without previous division, which I maintain to be analogous to the Pycniden of the nearly related Erysibe-species.

Result of the Cultures with Vaccine Lymph.

The Fungi-forms which come from the Micrococcus of kine-pock matter, by means of sporoid-forming and sprouting of sporoids, belong altogether to one species, and form the generations:

1. Of Acrospore, Aspergillus glaucus Lk.
2. Of Thecaspore, Mucor mucedo Fr.
3. Anaërophytic spores, Ustilago carbo Tul.
4. Fructification, Eurotium herbariorum.
5. Pycniden, and its subordinate Morphisms:
   Oidium lactis. Torula rufescens Fres.
   Ascophora elegans Corda. Botrytis Jonesii Berkley.
   Oidium albicans (Cladosporium).

Compare with these the next related fungi-forms.

1. Of Acrospores,
   Penicillium crustaceum Fr.
   Botrytis elegans Corda.
2. Of Thecaspores,
   Mucor racemosus Fres.
   Rhizopus nigricans Eh.
3. Anaërophytic spores,
   Tilletia caries Tul.
   Tilletia (lolii Tul.).
4. Fructification.
   Achlya. prolifera.
   Prings.
   Pleospora herb. Tul.
5. Pycniden.

That the morphism shows also a resembling parallelism is evident.

But the question is, which generation or morphism furnishes the Micrococcus of kine-pock? First of all, one affirms, with some assurance, that Mucor mucedo and Pycniden have nothing to do with these Micrococci, because they do not spring up directly from them, not even on a favorable soil: nevertheless they readily appear sometimes as soon as the Micrococcus has vegetated on nitrogenous soil, and after that on dry soil (cork and lemon). Aspergillus and Eurotium hardly furnished kine-pock Micrococcus, for they form, in liquid, as I could often show for Aspergillus, very active Micrococcus, and closely, as with Penicillium, by means of the continued division of spore-nuclei and bursting of the spore-wall; but this Micrococcus is quite colorless, not from wine-red to reddish-brown color as that from kine-pock. Torula rufescens Fres. forms a reddish colored Micrococcus. I have for a long time shown this fact, but yet the matter would be better proved by new experiments; and therefore Torula rufescens was first sown on various liquids and then on boiled human excrement and boiled neat's-flesh.

The forming of Micrococcus was energetically going on upon nitrogenous soil. The discharge of the manifoldly divided nucleus is, as was above figured, concerning Aspergillus. The Micrococcus is deep-red, especially when seen

in mass. From the hard nitrogenous soil the Torula sprouted and developed Macroconidia and Mucor mucedo Fres. with sporangia, sporangiola and panicled Botrytis.

Further, I sowed Ustilago carbo Tul. on the same substances.

The Micrococcus I have already described, which have no resemblance with them in color, but are always dark brown.

On the flesh and fæces the sprouts formed Torula chains, afterwards Macroconidia and Mucor mucedo Fres. Thus it appears evident that the Torula rufescens Fres. furnishes the Micrococcus of kine-pock. Now it is of the greatest interest, that this fungus appears very abundantly in milk, perhaps always in colostrum. I have shown in the case of swine, that the Micrococcus in colostrum for the most part belongs to Torula rufescens Fres. Rarely if ever does primary kine-pock show itself upon oxen, and in cows it is mostly confined to the udder; hence the conclusion that the cow becomes infected from its own milk, which conclusion is strengthened by the fact, that the disease most frequently occurs upon the cow's calving.

It is now my duty to state, as I learned by correspondence with Dr. Bender, of Camburg, that this distinguished observer, in 1859, had seen the vegetable organisms in small-pox; that he arranged the first culture and that his experiment was rewarded with success.

He wrote me Nov. 3, 1867, as follows: "As in the year 1859 compulsive revaccination was ordered, the peculiar form which was observed, brought me to the view, that in and with small-pox matter there must be a vegetative process; and therefore I commenced microscopic observations of the lymph, which I had in abundance. Commonly I found, beside the usual morphologic elements, as epithelium-cells, flakes of filamentary stuff, little hairs, &c., fragments of sharply-defined hyaline threads of .009 m. in breadth and

of variable length; twice there were seen larger balls, in great numbers, in the matter of the small-pox pustule, which on the employment of a solution of potash, ether and ammonia, proved to be spores.

"A small drop of vaccine lymph with sweetened water was placed in warm air, which had been filtered through cotton; four days after, a multitude of quickly moving points appeared, whose motion was stopped by acetic acid. Fourteen days after, I succeeded in raising threads which greatly resembled the Oidium of aphthæ."

Dr. Bender, nine years ago, obtained a morphism of small-pox fungi by culture, and the entire conformity of results from experiments instituted at various times, certainly speaks well for their certainty and correctness. For Dr. Bender, from his cultures, obtained no other thing than Cladosporium belonging to Aspergillus-Ustilago-Eurotium, and Mucor mucedo Fres.

IV.

Vegetable Organisms in Human Small-Pox.

For the material of this labor I am indebted to Dr. Reiter, of Munich. It was enclosed in a small tube and used for sowing as before.

First Culture. A sowing upon the white of egg, in the culture-apparatus, was made on the 9th of December, 1867. In March, 1868, the Micrococcus was greatly multiplied. Higher fungi-forms did not develope. The white of egg, at first golden-yellow colored, became at last of a brownish-yellow.

Second Culture. On the 6th of January, 1868, the contents of a small tube was sown on boiled milk, and put

into the isolating apparatus. It was provided with a glass tube bent downwards, and the apparatus remained unopened until the 24th; when, on the surface of the milk, was found a large number of Arthrococcus lactis (T. 2, Fig. 4), in all stages of development, the articulated joints mostly somewhat curved, resembling exactly those found in the vaccine lymph sown on milk (T. 1, Fig. 55). At the cork was found a beautiful vegetation of typical Aspergillus glaucus Lk. (Fig. 3, T. 2). Also coming from the small-pox matter on the cork, in the milk culture. Likewise a bastard between Penicillium and Aspergillus was developed, and thus (T. 2, Fig. 5) produced a spore-head of Aspergillus, which bears some branches, with chains.

Third Culture. On the 8th of January, 1868, the contents of a small tube, on a pealed and sliced lemon, was put into the culture-apparatus. In the second week after the sowing there was to be seen, on the white part, a small furze of a typical Cladosporium, as is shown (in Fig. 6, T. 2) under a strong lens. There was here the same Cladosporium coming from Oidium albicans; but it did not remain so plainly as at the sowing of the kine-pock on the same soil. The Ustilago-spores developed, in part, imperfectly, as Fig. 2 shows, but in a larger part (T. 2, Fig. 7, *u*) they form a dense plasma and become independent. Chains of such cells form a dense fur (T. 2, Fig. 6), from which was produced very numerous branchlets, partly barren (T. 2, Fig. 7, *f*) partly bearing chains of Cladosporium (T. 2, Fig. 7, *c l*). Other analogous threads bear still other fruit-forms. These threads are either a little branched or not at all, but terminate in a single, large, at first globular cell (T. 2, Fig. 7, *p*). Frequently on the end of this cell there is another mitre-formed (T. 2, Fig. 7, *p m*). This cell has two different forms of development, to wit, more or less divided in one or two directions (T. 2, Fig. 7, *s p t*),

and becomes by that means Sporidesmium-Stemphylium-fruit, already known by me as Aspergillus-Ustilago. At some spots the terminating cell was not divided, but contained about eight free reniform spores, or rather sporidia (T. 2, Fig. 7, *s p*, *p p*), resembling those of Erysibe and Pleospora, as have become known through Tulasne.

These forms, which indeed were imperfectly developed with their twin-nucleated sporidia, resemble, in part, the Pycniden of Erysibe, particularly the very young Pycniden of Erysibe Tuckeri Berk: so much so, that I did not doubt concerning the close relation of the genera Erysibe and Eurotium, or hesitate to regard them as analogous. Possibly the Pycniden of Eurotium wants the enveloping cell-membrane, which distinguishes that of Erysibe, and still more that of Pleospora; but yet, possibly, it came only on this soil to imperfect development.

Thus it follows from these cultures, that the micrococcus of small-pox is derived from one and the same species as that of cow-pock, but that it comes from another Generation, viz., from Stemphylium-Pycniden plants. I did not succeed in oft-repeated cultures with kine-pock, by means of Micrococcus on lemon, to beget Pycniden plants. I saw standing out within the lemon reddish-yellow mycelium with cells containing drops of oil, without observing that any fruit had developed.

Fourth Culture. On the 7th of January, 1867, (probably '68) there was put on starch-paste, prepared with acetate of ammonia, the contents of a small tube from Dr. Reiter, which was placed in the culture-apparatus. There appeared on the 27th of January, bastards between Penicillium and Aspergillus, and from the inner part of the substratum Mucor threads, which at the middle of February had not fructified.

Fifth Culture. On the 7th of January, 1868, the

contents of a small tube was put on sweetened water, with phosphate of ammonia, and placed in the culture-apparatus.

There developed, at the brim of the culture-vessel, a rich vegetation of bastard Penicillium-Aspergillus. On the cork appeared, at the end of the third week from sowing, a fine typical Aspergillus, which at the middle of February covered the entire surface of the cork. Within the fluid there began at this time the forming of younger spores of Ustilago carbo Tul, while the fructification of Eurotium had commenced at the cork.

For verification, I put a slice of well disinfected cork across the mouth of the culture-vessel. Eight days after there were to be seen small white flocks on the surface of the cork. They originated from Micrococcus-cells which had fallen on the slice. These greatly multiplied and formed circular Sclerotium, like those seen on hair and which Dr. Beigel sent me, which heretofore were taken for Algæ. I have named it Sclerotium Beigelianum.

I have a Sclerotium from small-pox culture, marked Fig. 8, T. 2, as it appears in a young state. Later the cells were seen to be enlarged; those on the wall of the vessel germinated and produced a fine typical specimen of Aspergillus-Eurotium.

Sixth Culture. On a well disinfected cork was put the white of egg on the 23d of January, 1868. Fourteen days after there was to be seen on the cork Torula rufescens Fres., beside the macroconidia of young specimens of Mucor mucedo Fres.

Result of the Cultures with Micrococcus of Small-Pox.

The result of the above cultures, nearly the same as that from the lymph of the vaccine disease, is remarkable, viz.:

that under the same circumstances, and from the same soil, the same generations or their morphisms appeared; i. e. on a dry disinfected plant soil, stand Aspergillus and Eurotium; on the fruit-peel of the lemon, Cladosporium-Stemphylium; on dry soil rich in nitrogen, Torula rufescens Fres. with macroconidia from which come Mucor mucedo Fres. But we cannot immediately, on the fruit rind of the lemon, get Pycniden from the Micrococcus of kine-pock; it is to be first cultivated on a nitrogenous soil (white of egg), and then transplanted on the lemon.

While the infection with the vaccine-fungus probably belongs to Torula rufescens Fres., which is to be considered as the younger progeny of Ustilago carbo, the Micrococcus so abundant in milk and even in colostrum, that of the small-pox probably proceeds from the Micrococcus which is developed from Schizosporangia (Sporidesmium-Stemphylium), and which always makes its appearance in company with Pycniden.

This result is therefore of the greatest practical importance, because the action of vaccination is perhaps thus best transmitted, when kine-pock and small-pox are derived from the same fungus; to guard against the small-pox is nothing else than to infect with the same disease, if the Micrococcus is the contagion. The great mystery of vaccination is thus explained—that one who has had the small-pox cannot have it a second time.

V.

Vegetable Organisms of Measles.

For the material of this labor I am indebted to the kindness of Prof. Gerhadt and his assistant, Dr. Schneider. On February 3d, 1868, I received the sputa, and on the 14th the blood of those sick with measles.

Investigation showed in the sputa tender Micrococcus in abundance, and, among these, other spore-forms, resembling fungus-cells, some globular, some long, of the size of a Penicillium spore and larger. From what fungus the colorless spores and Micrococcus came, could not be determined. Fungus-cells and Micrococcus were always found in the sputa, though not always in so great number. Fresh blood contained similar Micrococcus cells, though not in great numbers.

By arranged cultures the following facts were obtained:

First Culture.—Some of the sputa of a patient, sick with measles, on a peeled and divided lemon, on the 3d of February, was put into the culture-apparatus. There was evolved from the sprouting spores (later by the sprouting of sporoids), on the 7th of February, a vegetation of Penicillium crust. Fr. beside a Mucor-like fungus, which, however, was entirely suppressed by the Penicillium. The appearance of these fungi was not to be wondered at, but rather to be anticipated, because the Micrococcus and often also the spores were always to be found in the sputa.

Second Culture.—The sputa of measles with white of egg, on the 3d of February, was put into the culture-apparatus. On the 14th of February, at the edge of the white of egg, fine typical specimens of Oidium albicans Auct., developed to ripe Cladospores, from the Micrococcus swollen into sporoids, which sprouted and formed these Cladospores. To ascertain whether these Cladospores actually belonged

to Aspergillus-Eurotium-Ustilago-Mucor, &c., I placed on the same day a slice of pealed apple in the culture-vessel, and had the pleasure, on the 18th, to find a brilliant vegetation of Mucor mucedo Fres. and its preceding forms.

Third Culture. On the 3d of February, some of the sputa on paste, prepared with phosphate of ammonia, was placed in the culture-apparatus. On the 10th the surface showed a vigorous vegetation of Mucor mucedo Fres.

Fourth Culture. For verification the blood of one sick with measles, with paste and phosphate of ammonia, was put into a large isolating apparatus on the 4th of February. On the 10th I saw a vigorous vegetation of Mucor mucedo Fres. I remark here that not a trace of Penicillium or any other mould-fungus was to be seen until the opening of the apparatus, which, certainly, was very good evidence of the perfect closeness of the apparatus for experiments of this kind.

Thus the Micrococcus of Mucor mucedo Fres. is found in the blood and sputa of measle-patients, and the nurslings of this generation come directly through cultivation only, if we except the insignificant morphe of Oidium albicans. There is here also later evidence that the Micrococcus of a fungus retains throughout its own specific peculiarities; that from it that species only, yes, often that generation only, can be directly produced, which developed it. Naturally it has still its own action upon the substratum, and there is throughout no botanical contradiction in embracing the opinion, that the Micrococcus of Eurotium-Pycniden produces the small-pox; and on the other hand, that the Micrococcus of Mucor mucedo Fres. begets measles.

Concerning the spot where the infection through measle-fungus takes place, we can say but little, because the Mucor mucedo Fres., which is less abundant than Mucor racemosus Fres., appears on various decaying substances not too poor

in nitrogen. On human excrement, as well as on that of different mammalia, it is not rare. It appears on milk, but not so abundantly. It can be artificially raised on fruit, but it is not so vigorous as on a soil rich in nitrogen. It is found spontaneously produced and very abundantly upon all kinds of fruit, but mostly on cherries and plums. Therefore it is very probable that the infection with measle-fungus takes place in the privy, for the infection requires a copious evolution of the Micrococcus of Mucor mucedo Fres., which takes place in the highest degree in nitrogenous substances, and which is inhaled in great numbers. The Micrococcus of Mucor mucedo Fres. appears to be very abundant in human excrement, so that the opinion is probable, that by this means the atmosphere of pernicious decomposition must operate, and that the infection is introduced into the system from the air of privies.

I have, for a year and a half, publicly proclaimed, that it is our duty in all cases to disinfect the sinks and drains, because I was convinced that the Micrococcus of various fungi causes various contagious diseases, and that the contagion, i. e. the Micrococcus, are introduced by inhalation or by the alimentary canal.

This, my present opinion, was to be verified step by step, because every new detected vegetable appearance found in a contagious disease was the Micrococcus of a fungus, and indeed of a well defined (or specific) fungus; so that here is the place more than any where else for the above cautionary remark—that, at all times and in all places, to say nothing of cholera, disinfectants must be used if we would prevent or at least lessen the danger of infection with noxious Micrococcus from the privy or potable water.

VI.

Vegetable Organisms in Hunger-Typhus.

Typhus exanthematicus, or Petechialis.

For the material used in this investigation, I am likewise indebted to the kindness of Mr. Gerhardt and Dr. Schneider. It was in blood taken on the 7th of February, at the temperature of 32° R.

The precursory investigation showed that brownish Micrococcus existed in the blood in great numbers, which here and there formed small brown Mycothrix-chains. Also larger spore-like fungus cells, whose origin and signification I do not know. Probably they are the articulations of Oidium-chains of the respective fungi which came from the Micrococcus in the blood.

The cultures had the following results.

First Culture.—On the 7th of February, on a piece of peeled and divided orange some blood was put and placed in the culture-apparatus. Already on the eighth day after, I saw sporoids forming and sprouting. The sprouts on the tenth day had taken the form of Monilia cinerea Bon. (T. 1, Fig. 32). Beneath the surface of the fruit-pulp macroconidia were formed, which on the thirteenth brought forth vigorous specimens of Rhizopus nigricans Ehr. (T. 1, Figs. 28, 40, 42).

Second Culture.—As on the 7th of February a sowing on a slice of apple was undertaken. Already till the 12th of February there was developed, as on the orange, from sprouting sporoids, the Monilia cin. Bon. with macroconidia, and from these came Rhizopus nigricans Ehr.

Third Culture.—A portion of blood, on the 7th of February, was put in sweetened water with phosphate of ammonia and placed in the culture-apparatus. At the end

of the month stout Micrococcus had multiplied, but no other sporoids appeared, as fermentation always hinders sprouting.

Fourth Culture.—On the 7th of February a mixture of paste, prepared with sugar and phosphate of ammonia, after the addition of white of egg, and some blood from a patient sick with Typhus exanthematicus, was put into the large isolating apparatus. Until the 19th there were formed in the substratum yellow spots, which exhibited under the microscope a rich vegetation of Monilia cinerea Bon. and macroconidia. The Rhizopus came only feebly.

Result of the Cultures.

There is thus found in the blood of Hunger-Typhus the Micrococcus of Rhizopus nigricans Ehr., which can be easily raised on suitable soil from sprouting Micrococcus swollen to sporoids. The infection with Micrococcus comes by means of rotten fruit, decaying succulent vegetables of every kind, and also through fæcal deposits. Therefore, the infection from the privy is possibly, but more probably through the use of spoiled vegetable food of various kinds, and this throughout conforms with experience concerning infection with Typhus.

VII.

Vegetable Organisms in Intestinal-typhus.

(*Ileo-typhus, or Typhusabdominalis.*)

For the material of this labor I am indebted to Mr. Gerhardt and his assistant Dr. Schneider, as well as to Dr. Gietl, physician to the King of Bavaria.

On the 14th of February, I received, from the clinic at Jena, some blood of one sick with Typhus abdominalis. It contained exceedingly small Micrococcus cells, mostly single, more rarely united in chains. On the 16th, some of the intestinal contents of the same patient was sent me. They contained an unusually large number of slender, partly swarming and partly quiescent, Micrococcus, with here and there large balls of cohering Micrococcus-cells, and more distantly, single, colored-spores mostly of the size of Penicillium spores.

On the 18th of February, I received from Munich, by the kindness of Dr. Gietl, the excrements of another patient who had typhus. These contained almost solely masses of Micrococcus, frequently adhering together in the form of balls. Spores and other fungus-cells were more rarely found than in similar material from Jena. They are in general of immaterial importance.

Small crystalline or crypto-crystalline, brownish balls appeared very abundantly in the Munich material, single or in a heap (T. 2, Fig. 9), often joined together, and then tolerably like the cholera-fungus, or easily distinguished by their physical or chemical properties. These crypto-crystalline forms I often saw in the intestines, and always in cases where the blood was undergoing decomposition, as in cholera-stools, ulcerated intestines, but never have I seen them in such abundance or in such masses as in Typhus fever.

The Cultures exhibited the following Results.

First Culture.—On the 14th of February, 1868, I sowed in the culture-apparatus, on paste prepared with phosphate of ammonia, blood from a patient with Typhus in Jena. On the 18th there appeared a tender furze, which, on the 22d, had formed a dense vegetation of Penicillium

crust. Fr., with entirely normal fructification. The sprouts were developed from sporoids, as I confirmed on the 18th.

On the 25th of February, there was to be seen at separate spots, exactly in the middle of the drop of blood, a specimen of a large and mostly dichotomous and ramified Penicillium grande, the same as is shown (in Fig. 30, T. 1) to belong to Rhizopus nigricans Ehr. Within the substratum also were formed macroconidia, and some specimens of Rhizopus.

Second Culture.—On the 14th of February, a small drop of the same typhus blood was put on a peeled apple and placed in the culture-apparatus. On the 19th a vigorous and normal vegetation of Penicillium crust. Fr. shot forth. Only at one place there arose from the blood a small spot of Cladosporium herbarium Lk. Within the fruit-pulp macroconidia were formed, which by degrees increased upon the surface in the form of a sickly pencil of Penicillium grande (T. 1, Fig. 30), which was soon overrun by Penicillium crustaceum. Till the 27th the macroconidia generated vigorous normal specimens of Rhizopus.

Third Culture.—On the 14th of February, some typhus blood on the white of egg was put in the culture-apparatus. Micrococcus multiplied largely in the albumen. At the surface of the vessel was to be seen in eight days an Oidium vegetation, which throughout resembled Favus-fungus, Achorion Schönleinii.

The swarming Micrococcus were put under an immersion lens 1-18 of Merz, which appeared as T. 2, Fig. 20, shows. The cells are round-pyriform, colorless and prolonged into a cilium (or tail?) which simply oscillated here and there. The cells sometimes exhibited a spiral and revolving motion. Also the large yellowish-brown Micrococcus of Rhizopus were shown, but these appeared in the culture only sparsely, without doubt because of the large number of Penicillium.

Fourth Culture.—On the 16th of February, some typhus-excrement with lemon was put in the culture-apparatus. On the 22d the lemon was adorned with a fur of fructifying Penicillium crust. Fr. At some places there appeared more vigorous and taller fruit pencils with oblong-oval spores, as I have frequently observed with bastard specimens between Penicillium crust. Fr. and an Acrospore morphism belonging to Rhizopus.

Fifth Culture.—On the 16th of February, was placed in the culture-apparatus, typhus excrement with the white of egg. The Micrococcus of Rhizopus and Penicillium rapidly multiplied, and this last named fungus, in perhaps fourteen days, was preponderant.

Sixth Culture.—On the 20th of February, some excrement from a patient slightly sick with the Munich-typhus was placed on lemon in the culture-apparatus. On the 26th was found on the lemon, a normal, but as if from sour soil, a somewhat softer furze of fructifying Rhizopus nigricans Ehr., and also some Penicillium crust. Fr.

Result of the Culture with Micrococcus of Typhus.

The result is remarkable, and is different from all the investigated contagions till now examined. In all the other contagious diseases thus far investigated, the Micrococcus was derived from one and only one fungus. Here two fungi constantly appeared, viz., Rhizopus nigricans Ehr., and Penicillium crust. Fr. Moreover the relation of one to the other was very remarkable: while the Micrococcus of Rhizopus was sparse in blood, the small cells of Penicillium appear in far greater number; in the contents of the intestine, the case was the reverse, the Micrococcus of Rhizopus appearing in excess.

It is clear that the investigation concerning the vegetable

organisms of Intestinal-typhus cannot be regarded as conclusive, but in so far as any conclusion could be drawn from these cultures it was :— that the far larger celled Micrococcus of Rhizopus in the intestine is the first and true cause of the infection with the fungus. This Micrococcus makes way for the far smaller celled of Penicillium in the vascular system, whither it can itself advance only in less numbers on account of the larger size of the cell. But the smallest of the cells of the Micrococcus from Rhizopus, remarkable for their brownish color, one finds very sparsely in blood, while the very small-celled, colorless Micrococcus of Penicillium here preponderates.

The fact, in all probability, is this, that the Rhizopus with its spores come in the intestines; that it forms Micrococcus, which produce a similar destruction in the tissue; that the small celled Micrococcus of Penicillium, which is always abundant in the intestine, possibly can enter into the vascular system. Thus the cause of Ileo-typhus is to be distinguished from Hunger-typhus, not in the specific nature of the fungus, but in the manner of its inception. In Ileo-typhus the Micrococcus of Rhizopus comes in the intestine, where it will cause destruction; in Typhus exanthematicus, it enters by the lungs and is carried to the blood. In Ileo-typhus, the decomposition of the blood is the product of the action of the Micrococcus of Penicillium; in Hunger-typhus, of Rhizopus.

If we compare these results with clinical observation, we become convinced that Ileo-typhus is occasioned principally by the filthiness of culinary or potable water and by the food mingled with the Micrococcus of Rhizopus, and that these are diffused through the water by the imperfection of the drains and canals.

I think the privy, through evaporation, must be credited not indirectly as participating in the production of typhus-

contagion : in Typhus exanthematicus the evaporation from human excrement, bad food, decomposing vegetables and decaying bodies of all kinds, engenders the infection.

We find the influence of potable water upon the infection with Intestinal-typhus, particularly subjected to examination in the excellent work of Gietl. The water of some pumps in Munich was found, according to Vogel, to be so strongly impregnated with organic matters, that for its disinfection, a litre of water to ten milligrammes of the permanganate of potash was required.

Gietl shows conclusively that the impurity of the water in the Munich pumps depended on the imperfect condition of the drains, &c., and that the infection of some of the pump-water with organisms and organic substances was the cause of the disease in houses thence supplied. He comes, therefore, to this conclusion, viz.—That Typhus in Munich is quite independent of climate, as also of the soil, and the water in its natural condition. How just is this conclusion, the surprising abatement of Typhus since the arrangement and adoption of new water works, shows. * * * *

VIII.

Vegetable Organisms in Cholera.

In my work* devoted to this subject, I have shown, that in Intestinal cholera appears one and the same closely definable fungus, whose yeast brings forth in the form of Micrococcus the greatest destruction and commotion in albuminous substances. The Micrococcus is at no time wanting in the discharges of the cholera-patient; indeed, it is present in

* Hallier. Das Cholera-Contagion. Leipzig, 1867.

large masses in the dejections. On the other hand, the occurrence of developed fungi, and, in particular, of their fruit, which I have compared to the fruit of an Urocystis (Polycistis), are somewhat incidental; which, no matter what we may think of the fungus question, appears not to be in a necessary condition with the cholera-process. The Micrococcus is at any rate in that position to be multiplied indefinitely, so long as the necessary support is furnished it under favorable surroundings: the fungus fruits might thence, in consequence of their multiplication of the Micrococcus, aggravate the diseased condition of the intestinal wall; at any rate the latter sufficed to induce dangerous decomposition in the tissues. It is probable, in regard to the so seldom appearing fruits of the fungi, that they occur on the wall of the intestines; yet the attacking point of the Micrococcus and its action upon the human organism is not to be discussed upon the botanical, but only on the *Pathologico-anatomical and Pathological side.*

I have only shown, that the Urocystis oryzæ and its Micrococcus are present in the intestines of persons sick with cholera, and that we can, at a high temperature and on nitrogenous soil, bring forth therefrom the fructifying fungus-form. But how the Micrococcus gets into the intestines, whether here is the point of attack, or whether its first appearance is in the blood, or whether it directly produces the disease, or constantly attends it, which is scarcely credible:—all these are questions which I cannot undertake to solve.

I have to add some facts which may favor the probability of the opinion that Micrococcus and contagion are identical; which is to be argued—first, from my investigations and cholera-stool cultivation of the epidemic at Halle in the latter part of the summer of 1867, whose results are closely like those made known in my work on "Cholera-contagion;" and, secondly, from the result of the rice-cultures, which

were supplied with the cholera-dejections, and which furnished the result, that a brand-fungus developed on the rice, which killed the plant, and that the fungus was the same whose fruit sometimes came in the cholera-stools, and which formed the Micrococcus of the dejections.

But before I enter upon this double series of investigations, it may be well to give a survey of what was already suspected of the nature of contagion, and which was accepted on the ground of remarkable facts.

One of the first promulgators of the opinion of the vegetable nature of cholera-contagion, perhaps the first, was Professor Gietl.

He, already, in the year 1831, at the first spreading of the cholera over Europe, advanced this opinion, which has since been developed more clearly and precisely in his writings. His investigations upon this subject began in the year 1831, at Berlin, and were continued during the epidemic at Breslau, Ratibor, Troppau, Olmütz, Brünn, Vienna; in the year 1832 in other regions of Bohemia, and in 1836–1854 in Munich. Upon the epidemic at Munich, in the year 1854, he prepared a special report.

Already the chronological order of the breaking out of the cholera in the city hospital at Munich, indicated the transport of the disease from person to person. Clearly was this doctrine found advanced in the paragraph upon the "Cause and essence of cholera."

"The cause of cholera is a specific poison, which is of an organized nature proceeding from the dejections, which perhaps go through a peculiar process of fermentation, and spread from the dejections through the air among the population of a town and district."

"This poison grows exuberantly in the mucus of the intestines, particularly the small intestines and the stomach, and begins to produce decomposition and decay. The bodies

of the sick and their corpses have throughout no poison or infection. Only the evacuations of cholera-patients convey the seeds of the disease where they are allowed to remain. This poison is always introduced, and never appears spontaneously. From its action it is evident that it is exotic, and its essence has nothing similar in known pathology. But the poison, in its mode of propagation, has a resemblance to the poisonous cause of dysentery and typhus (Typhus abdominalis or nervous fever). Concerning the dysentery, it is a well known fact that the infection comes from the dejections: concerning stationary typhus, I am convinced that it is a poisonous sickness, and that the poison may be developed and propagated by the evacuations and modified places (Decubitus); probably, also, the excretions can strengthen the virulence of the poison, according to the different stages of the disease, and by the process of decay."

Furthermore, we read at p. 8—"What is the condition of that which is suspended in the air and coming from the evacuations, is unknown. All sorts of hypotheses are advanced thereon: but one idea cannot be relinquished, that it is an infinitely small organic body, the spores of cryptogams, mould-like, and is imperceptible to our senses aided in every possible way."

Further, at another place, page 9:—"We come to a conclusion concerning the nature of the poison, only from its action upon the human body: wherefrom we see, that this organic-stuff, this sad visitant, has a certain duration of life, is not always possessed of the same strength, and that there must be certain conditions for it to ripen, and for its action to take place. Perhaps not immediately upon the discharge of the contents of the intestine does the poison present itself; but it requires still a certain time for it to be propagated. This organic body contained in the cholera-poison appears to exist a long time, and under certain circumstances (as moisture and warmth) it can again be set free."

Highly interesting is the return of typhus symptoms, when the patients are attacked with cholera, as Gietl remarks pp. 10, 11.

Further, toward forming a correct judgment upon the vegetable nature of contagion, the communication which Gietl has made, in confirmation of the investigations of others upon the predisposing circumstances, is of the greatest importance. He says—"All that irritates or weakens the mucous membrane of the stomach or intestines, or that puts the intestinal capillaries into diseased action, let it be through the medium of nourishment, foreign matters, changes of temperature, individual conditions or mental influences, favors and supports the reception of the poison and its efficacy in various degrees. Evidently all sip in whatever of poison is diffused through the air, which produces, in the greater number, the gastric symptoms present,—as oppression at the stomach, flatulence in the bowels, disturbance of the digestive process and modification of the fæces, and with a smaller number produces the more advanced grade of the disease."

In relation to the cholera disposing circumstances, the most conclusive and convincing is the experiment, which Dr. Ehrlich in Breslau and other physicians undertook, in swallowing cholera dejections. The support of this heroic experiment remains very sound. Had they, beforehand, contracted the intestinal catarrh, the result would have been very different.

The botanical hypothesis that the contagion of cholera is nothing else than the Micrococcus of rice-brand fungus, renders a predisposition necessary; did not the predisposition exist, or were it the same with all persons, every one living amid the cholera atmosphere would become sick, and the disease introduced from Asia would not disappear. In general, every fungus which has to attack and destroy animal

tissue, must find it predisposed, and this in a particular degree can be maintained in relation to parasitic diseases of the skin.

The Favus, it is well known, is strongly contagious, but the artificial transplanting by no means always succeeds. In experiments upon my own person I succeeded but once, and then only imperfectly.

The Favus, imperfectly developed, healed wholly spontaneously, without the aid of any parasiticide. With some persons the experiment easily succeeds, while with others it fails entirely.

It is the same with regard to the mucous membrane. In diphtheria and croup there are fungi-forms, whose spores go to the bodies of the healthy without harm. With such they never come to development on the mucous membrane. On an inflamed or weakened mucous membrane, on the other hand, they sprout and bring forth masses of fruit and yeast. These formations certainly are not inconsiderable for the course of the disease: not without foundation does one bring a parasiticide into use. Just as surely for the development of the disease is a mucous membrane predisposed by catching cold or any other cause. Closely so is it with intestinal diphtheria. Should cholera and typhus be differently suppressed?

I cannot leave this idea, without calling attention to the following diseases which occasionally follow the cholera:

Uræmia.
Cholera-typhoid.
Bright's disease.
Diphtheria.
Pyæmia.
Cholera exanthem, &c.

* * * * From my work on "Cholera-contagion," I must regard it as proved that in cholera dejections appears the Micrococcus of a certain fungus not a native of Europe.

I must explicitly state, that a great number of cultures which I made with the material of Halle, under the same circumstances, have furnished the same results. Very important is the question as to the distinction between the origin of Asiatic cholera and common cholera (nostras).

* * * * These fungi-generations were probably introduced with the wheat-culture. Upon the wheat is found only the blight or wheat-brand, Tilletia caries Tul. These appear with three generations, to wit, Penicillium crust. Fr., Mucor racemosus Fr., and Achlya prolifera, as they were described by Pringsheim. All these we can, by a change in the physical and chemical qualities of the matrix, bring up from Tilletia.

While now the wheat, as is well known, is not of European but of Asiatic origin, so it is in conformity with the Darwinian hypothesis, that the Tilletia is introduced with the wheat and furnishes Penicillium, Mucor and Achlya.

The question now to be decided is of the greatest importance, viz., whether, at the usual summer temperature, the germinating Micrococcus of our cholera, was brought forth from something like the Urocystis oryzæ which I found in the Asiatic cholera. Therefore it was of the greatest value to me, that Prof. Vogel presented me with the dejections from some cases of PSEUDO-CHOLERA which could be used for culture.

It thus turns out, that at a certain summer temperature, 20 deg. R. or thereabouts, the cholera fungus does not come to maturity. The Micrococcus certainly sprouts, but produces normal specimens of Penicillium crust. Fres., and Mucor racemosus Fres., according to the matrix.

Still more cultures must be made before the question upon the distinction between the cause of Cholera-Asiatica and Cholera-nostras can be decided; but the result, mean while, is important, because it shows, that in the case in

question only native Fungi, not of the Asiatic form, but nevertheless Generations of the same species, appear in the intestines.

That here the Micrococcus, probably of Tilletia, acts, we may suppose, as contagion. The difference in its mode of action is only this, that the Micrococcus of Asiatic-cholera, i. e., of the Urocystis oryzæ, acts more energetically and destructively and in smaller quantity. On this ground the Micrococcus of Cholera can act easier than the Micrococcus of Tilletia by one inspiration of the fæcal emanations, while repeated inhalations of the Micrococcus of Tilletia from the cloaca, or their introduction by potable water, is necessary for the production of Cholera.

For either disease an investigation of the eventual vegetable forms in the blood is indispensable. This will furnish pure culture, and thence with greater certainty furnish an answer to the fungus question.

The question upon the causal force which lies at the foundation of Cholera (nostras) and Asiatic-cholera, leads naturally to the question of the predisposing force. That individual predisposition is present, has long since been proved by medical investigations. Wherein this consists, cannot be the subject of my discussion; much more have I to leave the investigation of these diverse and complicate conditions to pathology and pathologic-anatomy. But beside the individual predisposition, there is a general disposition present, and this for Cholera-Asiatica and Cholera (nostras) is one and the same, and is of the greatest importance. How remarkable is the graphic representation in the ninth report, where the curve representing the diarrhœa almost always rises and falls with that of Cholera.

Already, by this means alone, it has been settled, that Asiatic cholera is exotic, that it must differ from our cholera, which is very prevalent in years when the Asiatic cholera

does not appear. But sure is it, that the same miasm (pardon the word) which produces and enhances our cholera, intensifies also the disposition to Asiatic-cholera. Here, however, two different things are to be discriminated: (1) the real cause of our cholera, vegetable miasm or contagion itself; (2) the living condition of this contagion, whereunto warmth belongs. Hence the curve rises or falls more or less with the temperature. That a perfect parallelism cannot be found between the curve of temperature and the continuance of the disease is perceptible; then the temperature is not the cause, but only the means of its vitalization. That the cholera is not necessarily extinguished in winter needs no particular assertion; then always in the intestine is to be found the temperature necessary to the increase of the Micrococcus; not only in the heated rooms but in the heated houses in Russia the temperature suffices to keep alive the Micrococcus and to afford means of infection through the least filth. While now in summer, amid circumstances favorable to life-engendering, the Micrococcus of Tilletia in great numbers mix with the air and soil, so this Micrococcus, which probably is the cause of our cholera, must intensify the disposition to Cholera-Asiatica, for every reinforcement of fermentative precedents in the intestine must likewise strengthen the disposition to the cholera.

I am too far off to follow the distinguished PETTENKOFER'S investigations *pro et contra* in all their particulars, but this much I adopt as entirely conformable with my investigations:—that the conveyance of Micrococcus from the privy into the potable water and into the air and soil, not simply of Asiatic cholera but of our cholera, diarrhœa, and probably of other diseases, and which appear to be present in masses from Penicillium in healthy human intestines, and the Micrococcus from decaying substances in general; all furnish a disposition to Cholera-Asiatica. The Micrococcus

of Urocystis oryzæ passes through the entirely healthy intestine without mischief, but if fermentation in the intestinal contents is already advanced to an unusal degree, then the Micrococcus finds a favorable soil for germination.

By far the most important result of my later investigations upon Cholera-fungus, is the success in the culture on Oryza sativa. Already at the appearance of my "Cholera-contagion," I could show how the Micrococcus, scattered through the soil by the rice-water stools, enters the sprouting rice-plant, and passes through its tissues with a slender mycelium. Likewise, that the Micrococcus mostly swells before sprouting, forming sporoids.

The culture was continued until the end of October, 1867, and gave surprisingly favorable results. The rice covered with the cholera-dejections in the course of the summer appeared wholly abnormal under the influence of the mycelium spreading through the tissues. The leaves remained pale, chlorotic, and scarcely attained half their healthy breadth. Cotemporary rice sown without admixture or with Penicillium spores brought forth healthy sprouts and normal plants.

There appeared at last on the rice-leaves a blackish stripe, which proceeded from the point to the base of the leaves. Here the fungus fructified. I have designated it Urocystis oryzæ, placing it provisionally among the Urocystis, since the characteristics of this genus (Polycistis or Urocystis) throughout suits it. In the mean time it must be remarked, that this family is slightly known, and that fungi are arranged in it which have only slight relations with each other that can be confirmed by Generation changes. * * * *

Thus the vegetable nature of the small bodies in cholera-stools, is in every way confirmed and illustrated. It is only necessary, now, to investigate the Ustilaginia, which in India appears on the rice, in order to ascertain from experi-

ments, whether the Urocystis oryzæ exist in greater numbers and whether they generally appear. From a notice in an English newspaper it would appear that the cholera-fungus is found in India; but as the notice is anonymous, it cannot be regarded as scientifically authentic.

Lately various plausible opinions have been advanced in relation to the spontaneous motion of the small organisms of cholera-stools. This difference of opinion does not now exist, but rested entirely on a misunderstanding. That the Micrococcus in cholera dejections was self-moving, was indeed doubted by no one. Already, B. Paccini had spoken of small infusorial-like bodies. But it is an entirely different question, whether one has to regard as essential those provided with tails, or those yellowish and motionless Micrococcus cells which occur in great number. I am myself mostly in favor of the latter view. The swarming Micrococcus of the cholera-stool belongs to Penicillium crust.; the fæces never lack them, although they are not always in great number, while the yellowish and motionless Micrococcus cells, without exception, are most abundant.

But I have more striking, yes, convincing evidence for my assertion—that not the swarming, colorless, but the quiescent, mostly yellow Micrococcus cell is the contagion, if there is generally a vegetable contagion. In the rice-culture, with Urocystis oryzæ, there appeared from the spores of cysts (Schizosporangia) set free by means of a gelatinous swelling of the spore-wall, a motionless Micrococcus, and this it is which introduces the profound destruction.

I will not deny the possibility, that this Micrococcus can take the swarming condition under circumstances unknown to me. In every case, the swarming is, as with every other swarmer, only a temporary condition. There always appears, before the sprouting or dividing, a quiescent condition, and the tails vanish away.

Perhaps some remarks concerning remedies and disinfectants will not be unwelcome. Unfortunately I have had no time to multiply my earlier experiments upon the destruction of the Micrococcus of cholera-dejections; especially do I regret that it was not possible for me to cultivate the Urocystis in the presence of etherial oil, because this, as stated by others, had so great practical results. When the etherial oil is present, the development of the mould-fungus is hindered. But still it does not follow that this would be the case with every fungus, as cholera-fungus: the effect of every etherial oil upon every fungus must first be studied, before we can reach a complete induction upon this point.

In my work ("Cholera Contagion") I have reported a fine result upon the cholera-fungus with the solution of sulphate of quinine, acidulated with sulphuric acid, and can now with pleasure add, that I have received from several physicians notices where the efficacy of the quinine is confirmed. Dr. Methner of Breslau and Dr. Leib of Vienna, in particular, report favorably upon the use of medicines where quinine was the chief ingredient.

I am convinced that a more important result is yet to be attained, when the introduction into the system of the acidulated solution of the quinine is effected simultaneously by the stomach, by the bowels and subcutaneously: the dose being apportioned according to the stage and severity of the case. When used internally, either by the mouth or by the bowels, the dose should be from 2 to 5 grs.

In disinfecting the close-stool, I maintain, after my experiments, that the sulphate of iron is the best disinfectant. I have already shown in my work on the "Phenomena of Fermentation," that Mr. Pettenkofer, by the introduction of this substance, has made a happy hit, since disinfection depends less upon *destroying* the fungus vegetation than *preventing* the Micrococcus fermentation. Also, at a later

time, some inquirers, through experiments, disbelieved the efficacy of the sulphate of iron, inasmuch as in its solution and even in that of arsenical and cupreous salts, yes, even in a solution of corrosive sublimate, delicate fungus-mycelium may not only form but continue to vegetate. But this mycelium is entirely harmless, as it neither normally fructifies or produces any yeast, so that from it nothing unhealthy can be produced. When, therefore, such persons obtain from their experiments such mycelium, no serious objection to the use of acid disinfectants can be founded thereon. It is true, we can with this mycelium in a fluid liable to fermentation slowly excite the fermentative process, but so long as the mycelium remains in the vitriolic solution it is entirely harmless. Mycelium appears in a weak alkaline as well as in a weak acid solution. For the entire destruction of all vegetation, which however is wholly superfluous, large quantities of alkalies or acids are necessary.

To disinfect fluids, the mixture of Mr. Severn, of Halle, is most highly recommended. This, as is well known, consists of lime 100 parts, coal-tar 10 parts, and chlorate of magnesia 10 parts. I mixed a liquid strongly inclined to fermentation with 24 drops of cholera-stool from Halle, and added 24 drops of Severn's mixture. After some months the liquid in the isolating apparatus was wholly clear and free of fungi. The incipient vegetation was enveloped in this disinfecting medium and settled on the matrix. Hence in all cases where the contents of the close-stool can be kept in a liquid condition, this (Severn's) mixture is to be recommended as a superior disinfectant, and in such quantity as to cover the contents of the close-stool for some inches, for if only a small part remains uncovered the fungus will continue to vegetate. * * * *

IX.

Yeast-forming in the Intestinal Contents and from the Mucus–Membrane.

On May 13th, 1867, I examined, in Berlin, the vegetable-forms appearing in the excrements of a monkey (Cercopithecus).

There were found, besides the usual discharges, vegetable remains, plant-hairs, vascular cells, epithelial cells, &c., large masses of Micrococcus (T. 2, Fig. 10), which, as I have established by many investigations, are always found in the human intestines and introduce the decaying decompositions of the fæces; yes, in every digestion are of essential importance. In the stool of the monkey the Micrococcus often appears in conglomerate balls (T. 2, Fig. 10, *d*), that is to say, the parent cell was still scarcely dissolved. Likewise delicate sprout threads (T. 2, Fig. 10, *a*) were here and there to be found, pretty frequent spores of Mucor (T. 2, Fig. 10, *c*), less often articulations of an anaërophytic fungus-form (T. 2, Fig. 10, *b*), that is, of a brand-fungus, quite exceptional spores and cells of various brand and rust-fungi. Never did I find the spores sprouted, as is the case in the human intestine, only when in an unhealthy condition. The Micrococcus showed, for the most part, the swarming motions, partly, it was found, at rest and in the act of forming articulations which often remained connected together as Mycothrix-chains, as normally appearing in the healthy human intestine.

On the same day the monkey was fed on cake made of meal, eggs, sugar and water, with the addition of Rhizopus' spores.

On the 14th the remnants of the discharges were still the same, although the monkey had received nothing else than

the cake and milk as nourishment; plant-hairs, in particular, appeared in great numbers. No trace of starch could be detected, therefore the meal-cake (after twenty-four hours) had not reached the intestine. The fungus-forming in the morning still showed little change. In the evening there were found the Mycothrix-chains (T. 2, Fig. 11, *a*) far more numerous.

The Micrococcus was frequently still enclosed in the parent-cell in various stages of forming (T. 2, F. 11, *b c*). Spores or conidia, as T. 2, Fig. 11, *d*, shows, were numerous and often in the first stage of sprouting. These changed their color but little on the addition of iodine. By means of iodized chloride of zinc they, as well as the fragments of chains and the Micrococcus, became of a light greenish yellow-brown color.

Still on the third day were found plant-hairs (Steinzellen), fragments of plant-cuticle, &c. Traces of starch could be detected by iodine; thus three days were required for these to pass through the alimentary canal, while the monkey seemed quite well.

Not until the fourth day after the first meal did I find the spores of Rhizopus (T. 2, Fig. 15, *a–c*) in great numbers. These had partly lost their granular contents (T. 2, Fig. 15, *b*), partly the epispore was torn and empty. Besides the spores appeared numerous conidia (T. 2, Fig. 15, *d e*); as usual they appear in the midst of a pulpy substance on the sprout threads of Rhizopus. On these could be followed the gradual conversion, by subdivision, of the simple nucleus into Micrococcus in all stages (T. 2, Fig. 15, *d e*). Besides these appeared Torula chains of lanceolate fungus cells. Starch granules, colored blue by iodine, were present in great numbers. The articulated chains, conidia and spores were colored by means of iodized chloride of zinc from a burgundy-red to a violet, as Fig. 15 shows. The

odor of the monkey's fæces was, during these digestions, very similar to that of human fæces.

A second series of feedings was undertaken with the same monkey, after a time, when, in the cake, instead of Rhizopus' spores the spores of Tilletia caries Tul. were put, which furnished a wholly analogous result. On the third day could be followed the entire development of the Micrococcus from spores and conidia, by continuous subdivision of the nuclei, examples of which I have given in T. 2, Fig. 19, *a–d*, which, on comparison with my history of the development of Micrococcus in Tilletia caries, is readily understood.

It was remarkable, that during the feeding with Tilletia spores, the monkey's excrements exhibited a grayish (not yellow-brown) color, of a pasty and ropy consistence, evolving a cheese-like odor.

With a cake of the first kind (with Rhizopus) a man likewise was fed. He exhibited, by stool, after the sixteenth hour the spores of fungi and Micrococcus, changing gradually to balls (Fig. 16, *a–c*) of Arthrococcus. This individual was constipated before the feeding.

As a consequence of preparing the cakes of the first series, I suffered under diarrhœa, while Micrococcus formations appeared in great numbers in my evacuations, as I have shown in T. 2, Fig. 12, *b–d*. Conidia, spores and tender sprout-threads also were found.

Still interesting was the appearance of what came from the mucous membrane of the mouth and throat. Here, by repeated inspiration of the spores, Micrococcus formed in unusual numbers, while I suffered a not inconsiderable catarrh.

The Micrococcus exhibited all the stages of sporoid-forming (T. 2, Fig. 13), so that I considered it necessary to destroy them by a parisiticide, and to guard against their sprouting.

The epithetial cells amid the mucus showed in the most perfect manner the outgrowth of the attacking Micrococcus cells into longer and shorter Mycothrix-chains (T. 2, Fig. 14).

X.

Latest Investigations upon the Nature of Micrococcus.

That the Micrococcus is developed in the manner specified, viz., from the contents of the spores and vegetable cells of determined fungi by means of the repeated subdivision of the nucleus or nuclei, no one after my numerous labors upon this subject can any longer doubt; especially so, since a conscientious observer, provided with a first-class microscope, can confirm what has been said thereupon. In fact, this confirmation has, to my delight, been made by botanists of the first rank. So, the development of Cryptococcus and Arthrococcus from Micrococcus, by the chemical change of the matrix, is easily proved, and has been confirmed by different individuals.

It follows that the Micrococcus retains its own specific nature, so that any one can now get only that fungus from it by means of the sprouts which begot it, as clearly appears from the above mentioned investigations. The most difficult point is the motion of the swarming Micrococcus. Here are needed the best microscopes of the best manufacture, the highest powers and the best illuminating apparatus. But all these suffice only when one brings into use all the artificial helps which the laws of optics suggest. Now not all microscopists are in such favorable circumstances, possessing instruments of the first class and from the various workshops, and thus there is a dispute in relation to the motions of

swarming Micrococcus. PACINI, KLOB, THOME and some English observers very accurately attribute to one portion of the small cells (Micrococcus) of Cholera discharges self infusorial-like motion, in contradistinction to molecular movement. Less expert observers, provided with an ordinary instrument, have reproached a certain person for his statements; while here, nevertheless, the use of an acid would at once decide, by the immediate suspension of all self-motion, while the molecular movement would be seen at first to be very energetic.

With a powerful system of a good microscope one will with sufficient clearness perceive the motive organs of the small swarmers.

With the system G of Zeiss, even with the system F of the same optician, by the help of a weak eye-piece, as with the immersion system No. 11 Hartnack, and with an immersion system 1-18 of Merz, it can be seen that in the Micrococcus (not indeed in every Micrococcus) of some fungi, especially of the Mucor-kind, the swarming-cell has a longer or shorter tail-like prolongation, as I have often exhibited in my designs. These locomotive organs are not shown so plainly by the previous systems as by an immersion system 1-18, recently procured of Merz in Munich.

* * * * * * *

With this system, by the help of ocular No. 4, I can perceive the tail, under the favorable western light of the morning or eastern light of the afternoon sun reflected from a still cloud to every Micrococcus, which, by its oscillation, produces its movement. That such is the fact (after very close focusing, arrangement of the diaphragm and mirror, the protection of the eyes against direct light by means of a screen, and that of the object from light from above by means of a small black shutter, and other delicate manipulations which are essential, as the skilled microscopist knows), would need

no confirmation, had not a false view been recently diffused, viz., that *only* two *sound*, *not experienced* eyes, were necessary; that to such everything must be plain, and that what such persons cannot through lack of practice find, they even deny the existence of, while it is readily seen by experienced observers. Had we not good microscopists, such observers would occasion more confusion than exists in the subject itself.

I will here give some examples of swarming Micrococcus which I had an opportunity to study lately. Moritz Willkomm, as already stated, had heretofore, and more recently noticed anew, the development of Micrococcus. He gives a description thereof from Corticium amorphum Fr. in his brilliant investigations upon the diseased bark (cancer) of the Larch, which he had the kindness to send me, together with material for investigation, so that I am in a position to compare his statements with my own research. I can corroborate that the spores of Corticium not arriving at maturity, and not capable of sprouting, develope Micrococcus in a swarming form. It appears, that the mycelium-cells bring forth Micrococcus, yet they are less easily and certainly to be identified. Still the sprouting of the ripe spores on the object-bearer I found closely as Willkomm had described and figured. I succeeded not only to develope from the double spores in a drop of glycerine on the object-bearer, the two end sprouts, as seen at T. 2, Fig. 19, *a*, but also with thinner sections through the Corticium to show the sprouts broken out even from the Asci, as seen in T. 2, Fig. 19, *b c*. This circumstance was, to me, of great importance in judging of the mould-form raised from spores by Willkomm.

It was Willkomm who succeeded in bringing up from the germs a pencil-fungus which he classed with Penicillium. As now the Penicillium already on the second or third day fructifies, so he succeeded here to follow the fungus of the

fruit-pencil backward to the sprouted spore. Willkomm sent with his preparations a drawing, which proved this, as I succeeded in doing, to follow back the same pencil-mould not only to the sprout spore, but even to the spores still covered in the Asci. But not more evidently could be proved the consanguinity of two fungi, and I was gratified to be able to ratify the brilliant observation of Willkomm.

Concerning the Penicillium, it is difficult to say, whether it should be regarded as Penicillium or Cladosporium. The fructification of both is so undecided and variable, that we can seldom make out for either a sure definition. The change of Generation is here the main thing. The Penicillium alluded to, has a brownish color, often it is dark-brown. The ramification is incipiently irregular, the spores or joints are long, spindle-shaped, sometimes divided or having septa, towards the end of the chain nearly globular, but generally with two very blunt-pointed ends. In the fluid the specimens soon became pale, and now resemble in some measure the fungus Penicillium crustaceum Fr., but in the form and arrangement of the spores there is always some difference, and the whole habit is different.

After a week's continued duration of the culture on starch paste with phosphate of ammonia, the fungus entered within the matrix; but here the joints become deep-brown, independent and fallen to pieces by division into a globular brand-spore. This brand-fungus, this anaërophytic form of Corticium, has entirely the figure and spore-form of an Ustilago. It strongly resembles the Ustilago carbo Tul., but it is somewhat distinguished by smaller and paler spores. Thus the consanguinity of three fungi-forms, which formerly were regarded as belonging to different species, viz.: Ascomycetes, Corticium amorphum Fr., with its already known change of generation, the anaërophytic brand-form; Usti-

lago corticii, and the appertaining acrospore-form; Penicillium, or Cladosporium corticii, is here again proved. * * *

The swarming Micrococcus of the Corticium spores (T. 2, Fig. 21) does not belong directly to the larger forms, but there plainly appeared, already, with the system of Zeiss, a tail-formed prolongation of the cell. With the immersion system of Merz, these tails appeared as plainly, as is shown in T. 2, Fig. 21. One sees, that these swing here and there, but yet most have a revolving and spiral motion also. Upon longer and closer observation we come to the conclusion, that the Micrococcus cell is contractile. In motion they change form considerably.

More clearly are these changes of form seen in the Micrococcus cells of Penicillium crust. Fr.; for example, in blood and the stools of typhus (T. 2, Fig. 20). One can here distinguish the cell from the tail. The motions of the tail are closely like those of Corticium. But the cell is sometimes globular, sometimes pyriform, sometimes stretched out, and one sees these changes interruptedly take place, or by degrees. In both cases here described, the Micrococcus is perfectly colorless. Not so with Rhizopus. This is naturally unimportant, whether one observes the Micrococcus from the spores of Rhizopus or from typhus-stool. The swarmers appear here under the immersion system and with ocular No. 4 as large and plainly as is shown in T. 2, Fig. 22. One sees a large yellow-brown cell wall, which, as the parent cell, surrounds the true swarm-cell. This is found free, central, or at the side in the parent-cell, through whose wall the long tail proceeds. Its length amounted to five or six times the diameter of the cell. It swings, in motion, here and there, and is ordinarily found before the cell, this dragging behind. Sometimes, in particular when meeting with obstruction, it shoved the cell forwards.

Much more distinctly is the tail seen in the smaller but

quickly moving Micrococcus cell of Mucor mucosa Fr. of the measle blood, as also in the Micrococcus from the spores of the genuine Mucor mucedo Fr. (T. 2, Fig. 23). I have figured some of these as I saw them under an immersion system of Merz. They are nearly colorless, very light golden-greenish. They move here and there very quickly, sometimes forward, sometimes backward, sometimes revolving. I mentioned before that purely vegetable cells are able also to develope Micrococcus, as Willkomm declares. Rarely do they seem to divide easily, whether the Micrococcus is produced from the spores or from the cells of Mycelium also. A good example of this is Oidium, which the mould-fungus puts forth. Here we often have joints, only, without a proper fructification. Thus T. 2, Fig. 29, shows some joints of the Oidium of Rhizopus, as if they were partly filled with nuclei, and between the joints the Micrococcus multiplying as coming from the liberated nuclei.

The older elaborators of the yeast-theory came to their false conclusions and dogmas by the false system of their investigations, partly through the erroneous explanation of what they had seen, and partly through their entire ignorance of the later labors of the chemist and physiologist in the field of the doctrine of Fermentation.

With nothing else than the isolating apparatus, and that imperfect, one cannot bring out the culture which I have discussed in my writings upon fermentation. But a lack of knowledge upon fermentation in general necessarily occasions mistakes in the examination of the yeast. I might here bring forward an example. Everybody knows that the representatives of the older doctrine upon fermentation were acquainted with only one kind of fermentation, the spirituous. Thence the childish criterion : if at the decomposition carbonic acid is developed, or when, more rudely expressed, gases rise, so the acting organism was yeast and nothing else.

All the other formation precedents are disregarded by such dogmatic definition; and while now the spirituous fermentation is by no means so plain an event, as we must believe after the just mentioned theory, so must errors arising be very many and evident, and the discovery of a developmental history formed upon a distorted apprehension, be an impossibility. This impossibility of discovering the yeast development upon the elder theory, is far more evident when the false method of inquiry and necessarily false explanation of the observations is taken into account. To wit, when one employs only a closed apparatus (isolating apparatus) for culture, and this first opened at the close of the culture;—thus it is impossible, *a posteriori*, to obtain a clear idea of the development of the active organisms taking place.

One obtains only the final product of the experiment, and it is a pure sport of fancy when one any way judges of and interprets the earlier condition of the organism exciting fermentation, which is permitted to take place only step by step. And now the limitation of spirituous-fermentation! Here lies, first of all, the danger of gross errors, viz., the confounding of cell-sprouting and spore-sprouting with actual yeast-cells, which are similar. The intermediate forms between genuine yeast and mycelium are naturally many, and such has too often been regarded as yeast itself until my investigations. Such sprouting spores, as have been very closely observed by J. Sander, come on the surface of substances undergoing spirituous fermentation, when the air is admitted, as can be no otherwise in the isolating apparatus earlier made use of.

I have, for example, some sprouting vegetable cells of Favus-fungus (Oidium of Penicillium crust. Fr.) as T. 2, Fig. 30 shows. A certain resemblance between such a formation and cryptococcus is incontestable. But I have already

earlier shown, in my treatise on "Fermentation," that such intermediate forms, viz., the transition of yeast-cells into mould-forming (for example, Hormiscium), or inversed transition of mould-form into the formation of yeast, only happens on the surface, never within the fermenting liquid. It needs only the examination of the bottom yeast to be convinced of this; but one gets it not in using the former apparatus.

Where would it lead if all cell-sprouting was to be regarded as yeast? Those conidia and spores appearing acrogenous, always sprout closely in the same manner from the stem-spot like the young cryptococcus cell.

If one compares only the spore-forming of Aspergillus, Penicillium and other fungi with the spore-forming of cryptococcus, he will be convinced that the whole process is the same. But it is nothing else than one of the two main forms of the cell multiplying of the fungus. T. 2, Fig. 31 shows two stem-cells of Aspergillus with spores; compare T. 2, Fig. 3, *s p*. At *x* is seen a very young sprout-like spore, at *s p* a similar one ripe. Fig. 32 shows the same in Penicillium. In Fig. 33 is figured a sprouting brand-fungus. Everywhere among the thread-joints are seen sprouting conidia breaking forth. Should all these structures be regarded as yeast-forming because they pass through a similar histiological development, as cryptococcus, or shall it not rather be asked, is this sprout-forming really yeast, and the cryptococcus the only yeast-form, or is there still another kind of yeast generation present?

I have long since answered this question, and as my yeast doctrine has enjoyed universal recognition and ratification, I have put far away the renewal of any conflict with the fancy forming of the old yeast doctrine. But I regarded is necessary to show to those ignorant of botany, that in the three above mentioned points a large source of error

and false conclusion lies hid, which must be known to be avoided.

When one undertakes actual cultures which are free from the above censured errors, he will no longer retain those old dogmas.

I have in T. 2, Fig. 35, furnished an example, as the conidia of the before mentioned brand-fungus (Fig. 33), in the liquid medium its Micrococcus developes (T. 2, Fig. 34, *a–d*) as also the parent-cell subdivides (T. 2, Fig. 34, *b c*, *f g*), and at last, when the wall-membrane was dissolved the Micrococcus came out of its original confinement (Fig. 34, *e–g*). On dry nitrogenous soil Micrococcus also forms, but in an entirely different arrangement and is therefore wholly mistaken. I have already made observations on dry meat in dry air, and found that the mould-spores fallen from the air on the meat did not sprout, but formed Micrococcus. If the air is very dry, the Micrococcus is in chain-form. The chains (Mycothrix) anastomose on the side and throw out exceedingly soft fructifying threads, mutually ramified by means of fusions, and forming a proper fur. I have already shown that the Mycothrix fungus bears a close analogy to the Sclerotium forms.

Later I have found real Sclerotium, which was formed by the Micrococcus coming from common mould.

The form of Sclerotium is uniform. It appears everywhere when the fungus cell of similar form and origin developes in such large numbers that they cannot form sprout-threads on dry soil, for want of room. So, for example, appears the (Mutterkorn) ergot. In the sweet sap of the flowers of the cereals and grasses, we find, at first, some single yeast-cells or conidia of Sphacelia segetum Lev. These sprout and form fructifying threads of Sphacelia, the fallen conidia of which now come in such masses in the before mentioned fluid, that they absorb it and have no

more room to develop Mycelium. They extend any way, and are augmented still by means of subdivision. Thus they form the large, spongy and at last solid body called Mutterkorn.

Closely do they resemble the Micrococcus of Penicillium and Aspergillus in their peculiar appearance, and they have been observed lately in the hair made use of by hairdressers.

Dr. Beigel, of London, had the kindness to send me such hair. It showed, here and there, knots (T. 2, Fig. 24, *a b c*) which embraced the hair wholly or in part, and already with low magnifying powers showed very tender cell-structure. The smallest and youngest knots consist of Micrococcus, which, in small heaps, covered the respective spots. In the larger knots are much larger cells (T. 2, Fig. 25), which have so flattened one another, that they form a false mycelium. It can now be plainly seen, that each possesses a large nucleus, and that it is about to be divided into two or four parts. Therefore the formation remotely reminds one of the quartering in the lower Algæ (Merismopœdia, Pleurococcus and others), but there can be no doubt, at first sight, that here a fungus is represented. To this its optical and chemical condition witnesses.

The cells in the compact knot exhibit, in their multiplying, great resemblance with those which I have called colony-yeast (Sarcina-form), and which, in fact, belong there, in so far as their origin is concerned. Only the aggregation is here different. By the close crowding together of a very large number of fungus cells, which are still in continual division, there is formed mycelium closely like ergot, i. e. a Sclerotium. Certainly it is proper henceforth to designate this knot "Sclerotium Beigelianum," when it is recollected, that Sclerotium is no especial fungus-genus, but a morphism appearing with many fungi. To find out to what fungus the

Sclerotium Beigelianum belongs, I cultivated the same, in air saturated with moisture. The Sclerotium, by and by, fell apart into its cells, whereby the parent cell was dissolved (T. 2, Fig. 26).

Each cell sprouted (Fig. 26, *k*) and brought forth, after a few days, pencils of Penicillium (Fig. 26, *p*). Among some few Sclerotium there formed only the sprouts of Aspergillus, so that we must grant a part in the knot-form to both fungi.

Therefore the whole process consists in this:—that the Micrococcus of Penicillium crust. Fr. and Aspergillus glaucus Lk., develope on the hair; that the Micrococcus cells swelled to sporoids, which did not come to sprouting, but, being crowded together in great numbers, were limited by subdivision, in one or two directions. Thus they formed Sclerotium.

It occurred to me to prove this explanation by an artificial nursery of this Sclerotium from the spores of Penicillium and Aspergillus.

I sowed the spores of Aspergillus on a well disinfected cork in the culture apparatus. On the cork I put some of my light hair. The spores which fell on the hair formed Micrococcus. These lay in very small heaps on the hair (T. 2, Fig. 27). The Micrococcus quickly multiplied by division, and formed large knots on the hair. In these the cells swelled to sporoids (Fig. 28), and now began the subdivision of the nuclei. This, thus formed Sclerotium, fell to pieces in a moist room, as is the case with Sclerotium Beigelianum; the single cells sprouted and brought forth on the hair bright Aspergillus pencils. (T. 2, Fig. 27, *s p.*)

XI.

Apparatus.

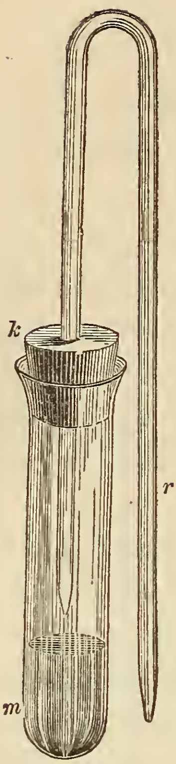

Fig. 1.

* * * The most simple apparatus used by Professor Hallier, is represented at Fig. 1, as copied from his work on "Fermentation," and there described. It consists of a glass test-tube, first boiled out and cleansed by alcohol. It is then provided with the previously heated matrix (*m*) and hermetically closed by the cork (*k*), through which the inverted U-shaped tube (*r*) was passed, which can be easily removed as may be desired, only taking care that it shall not touch the matrix. I now use this apparatus so that, before putting the cork in its place, the matrix is boiled in the tube for two minutes. For negative experiments (tests), the cork with the tube can be put in while the boiling is going on. Only in this case we must proceed very cautiously. I cleanse, before use, in the following manner. If the vapor, at 100° C., is allowed to escape through the drawn end of the tube (*r*), it may be regarded as clean ; but, for the sake of precaution, the tube is filled with pure alcohol, and allowed so to remain about an hour, generally until the matrix in the test-tube is heated and the substance to be cultivated introduced, which is accomplished as follows :—

The tube, filled with alcohol, is quickly emptied and as quickly dipped into the matrix with the same end which shall come into the vessel (or test-tube). Meanwhile the lamp beneath is blown out and the cork inserted as soon as the boiling has subsided. Where the substance to be examined is liquid, a certain quantity adheres to the entrance

of the tube if the outward opening is small enough. If the substance is solid, as, for instance, the scales and scurf of human exanthems, they adhere to the outer and inner part of the tube. This, at first, is pushed only so far down that it is found with the lower part still high above the matrix. The air ascending only from without through the bent tube towards the test-tube, by degrees a portion of the substance to be examined is pressed downwards; when the matrix is sufficiently cooled, through tilting and shaking, the adhering substances are brought in connection with the substratum.

I have made this apparatus of various calibre and form, and used it constantly with good results. With the greater number of substrata, I took a flask instead of the test-tube. PASTEUR says, positively and clearly, that a similar open apparatus gave as distinct and satisfactory results as the more complicated, and I have never neglected, when a substance was to be examined, to make controlling experiments.

* * * * * * *

If one would have pure and clean air, he must use a pressure or suction apparatus. I have used both kinds, but to the latter (the air-pump) I give the preference.

I employ compression by means of gasometers such as the chemists use. The air is passed by suitable pressure. In use the upper part of the gasometer is connected by means of an air-tight tube, which conducts the passing air through boiled cotton and sulphuric acid. The cotton is put into a larger tube which it fills for some centimetres. The sulphuric acid is put into the well-known sulphuretted hydrogen apparatus. The culture apparatus, a flask in this case, bears a U-formed tube, and the air escapes as I press more in.

Naturally we can pump in the air more directly through clean tubes into the apparatus; however, the above described method has the preference on account of a more equal pres-

sure. I have used lately and for greater convenience, half the suction apparatus. For this purpose an aspirator, in general, is used; still it has many inconveniences. I make use of the air-pump.

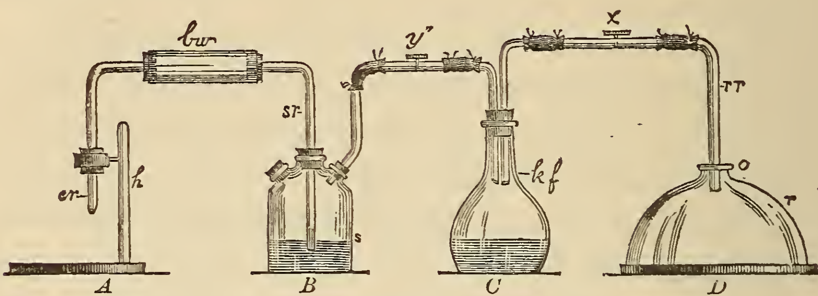

Fig. 2.

Figure 2 shows such an apparatus ready for use. C is the flask which contains the substance to be examined. It stands on the tripod of a Berzelius lamp. Its cork is doubly perforated for the bent tube. The matrix is heated in it, corked, for two minutes. Thereupon the substance to be examined is gently placed; afterward the glass tubes x and y are put in their places; these are capable of being closed by means of a stop-cock x and y. After the deposit the previously opened tubes are allowed to remain so. The tube x stands connected with the bell-glass of an air-pump whose plate is represented at D. A cork is adapted to the opening o, through which the tube rr, which is in connection with x, is passed. The tube y is in connection with the sulphuric acid apparatus s. The tube sr, partially filled with cotton, is introduced through the corked middle opening and terminates below the sulphuric acid, through which the air is admitted as the air-pump is worked. The sulphuric acid apparatus stands on the table B, as also does the holder h, which supports the tube containing the cotton, on table A. As a proof of the air-tight closure, appears the rising bubbles in the sulphuric acid, while the air-pump is slowly worked.

As soon as the substance to be examined is put into the flask the air is slowly pumped out of the whole apparatus, which is to be repeated once or oftener every day.

As is readily perceived, this apparatus admits of many modifications. Which to choose may be found explained hereafter. Still, I will here remark, that with aspirators or expulsors, it is better that the air should be passed through alcohol rather than through sulphuric acid, even in using the air-pump. In moderately concentrated sulphuric acid it happens that many fungi-elements are not killed, but actively vegetate, and it is questionable whether the acid in the highest state of concentration can kill them. The whole process must be regarded as a cleansing. At all events, filtration by means of cotton or other fibrous substances is indispensable; and then, even, the entrance of fungi-elements, by means of the air in the cleansing liquid, is not impossible.

* * * * For culture, to follow the vegetation step by step, we need a wholly open vessel, not a narrow-necked flask.

Very frequently we want to convey the organisms into the medium on the object-glass, sometimes under the glass cover. Here, then, is required a particular culture-apparatus, hence the following remarks. * * * *

In all the kinds of culture alluded to in my book on "Parasites," I have used the above specified apparatus, modified in a variety of ways; and now, for many years, I have from 20 to 30 of such apparatus in constant use in my various cultures, and have had occasion to use no other. The principle of the apparatus, in all its variations, is very simple. A flat dish (*s*, Fig. 3), is filled with water. In the water a small inverted dish with an even bottom is placed (*fs*, Fig. 3). On this, another small dish, *o*, is put, which contains the matrix and the substance to be examined. Over the object, the inverted bell-glass, *g*, is now

set in the water, so that the sowing is kept air-tight. Sometimes I use an open-mouthed bell-glass with the mouth closed by a doubly perforated cork (*st*, Fig. 3), which permits the air to be removed without taking off the bell-glass. One of the openings of the cork carries a tube (*rr*, Fig. 3), which connects with the recipient of the air-pump. Another tube (*kr*) is placed in the other aperture of the cork to connect the cleansing apparatus. For ordinary purposes both tubes through the cork are closed.

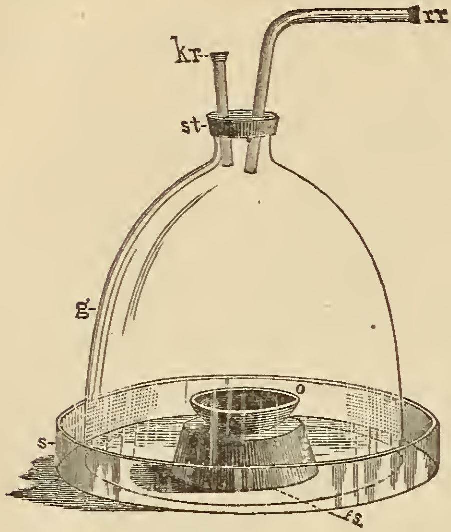

Fig. 3.

In this modification the apparatus has the advantage, that the air can be renewed at different times before opening the bell-glass. Which of the above described apparatus will come into use, and which will be preferred, must depend on each determined case and the taste of the investigator. * * Immediately before culture all the pieces of the apparatus, &c., should be well washed in alcohol and rinsed in distilled water; and the matrix, previously heated for two or three minutes and until moderately cool, set under a clean bell-glass.

XII.

Résumé.

General Outline of the Views entertained by Professor Hallier.

[Translated from an Essay of M. Baudouin, published in Professors Coze and Feltz's work upon "Infectious Diseases."]

"For a long time it had been believed that fungi, even the most inferior (moulds), might be divided into well characterized species. In 1851, M. Tulasne first discovered that the same species might bear very different kinds of fructification, and, that thus, many forms, which had been regarded as distinct species, were only modifications of one and the same species. A little later, in 1857, Bail pretended to demonstrate that ferments were no other thing than the most simple forms of certain moulds long known, as for example Penicillium. These ideas were adopted by Berkely and Hoffman. Hallier extended them farther. He thinks that he has proved that ordinary moulds reproduce diverse forms, and that these changes in the aspect of the plant depend on many causes: to wit, the free admission or exclusion of the air during their vegetation; its temperature, and the moisture or dryness of the matrix upon which the plant vegetates; the chemical nature of the elements upon which it feeds (hydro-carbons, sugar or azotized matter), &c. &c.

"I proceed rapidly to review the different forms admitted by Professor Hallier:—

"All moulds, even the highest forms, as Agarics, can act as ferment: he calls *Ferment* the forms or morphes which develope in the interior of a substance capable of undergoing fermentation (and consequently organic), of being nourished at the expense of this substance, and of producing

multiplied generations, which rapidly succeed each other, and always remaining one-celled. These ferments may come from the spores (organs of reproduction) of moulds, or, indeed, from the mycelium (that is to say, the vegetative organs). The moulds can present themselves under three different aspects :—

"A. That of *true ferment.* In this case, the globules remain separate, the one from the other, and are always suspended in the liquid.

"B. The *ferments in chains.* This second form, which M. Hallier calls *imperfect ferment*, is composed of globules agglutinated in filaments ; it appears when the air is imperfectly admitted.

"C. *Colony-ferment*—that when the first cell, instead of producing buds, quadruplicates itself (for example, Merismopædia or Sarcina) ; or, indeed, when it is composed of cells confusedly conglomerated.

"I proceed to examine what are the varieties met with in each of these divisions :—

"A. Of the true ferments there are three forms : that of Micrococcus, that of Cryptococcus, and that of Arthrococcus.

"1. *Micrococcus* (Microzyma of Bechamp). This form is that where the spore-membrane of a mould, or indeed, that of a joint of mycelium, breaks and allows the escape of the plasma. If this protoplasm meets with organic substances capable of putrefaction, the granules of the Micrococcus organize, and take the form of a small sphere very often provided with a pedicle or tail, which enables it to move (Swarmers), when they multiply abundantly. The Micrococcus may be regarded as the promoter of putrefaction.

"2. *Cryptococcus.* If, instead of coming in contact with a substance capable of putrefaction, the protoplasm of the spore, or the Micrococcus already developed, is in contact with a substance slightly azotized or apt to undergo

alcoholic fermentation, the form changes. The Cryptococcus developes very rapidly, not by division, but by budding.

"3. *Arthrococcus.* In fine, when the same organs already cited are put in a medium which has a tendency to take on an acid fermentation, they assume an elliptic, elongated form, and are called *Arthrococcus.*

We may hence conclude, that Professor Hallier's theory is just the opposite of M. Pasteur's. He believes, that for every fermentation, a special ferment is needed; for example, that the Micrococcus of milk is requisite to produce lactic-acid fermentation, while M. Hallier, on the contrary, regards the form of the ferment as depending on the particular kind of fermentation: thus, if we put Micrococcus in a solution of sugar, we shall obtain Cryptococcus; if we put it into an alcoholic liquid in contact with the air, we get the acetic ferment, that is to say, the Arthrococcus.

"B. One has only to suppose the diverse forms, enumerated in the first section, adhering together endwise, to have those of the second section.

"1. *Mycothrix.* These are no other than the filaments composed of Micrococcus, united together in a bead-form. This takes place when the Micrococcus is on the surface of the liquid, and consequently a little more exposed to the air, or when floating in an oxygenated liquid. These forms were formerly known as Bacteria or Leptothrix; but as the name Leptothrix had been previously appropriated to an Alga, Hallier prefers the nomenclature of Itzigsohn, which recalls the mycologic origin of this form.

"2. *Torula.* When the Cryptococcus are in the same condition as the Micrococcus, they produce like chains, only composed of larger joints, which have received from Mycologists the name of *Torula.*

"3. Mycologists have likewise given the name of *Torula* to

chains composed of the joints of Arthrococcus, and which appear under the same conditions as the two preceding.

"When the three kinds of chains exist at the surface of the liquid, they adhere together and form a kind of net-work, appearing to the naked eye as a membrane: this is the Mycoderm of Pasteur.

"C. Under this last division we have only the forms which extend to the surface, as Sarcina, or indeed which are divided irregularly in every direction, and form agglomerations.

"I pass to the more developed forms, i. e. to the MOULDS.

"*Moulds.* M. Hallier believes that the absence or presence of the air modifies their degree of development. Thus he divides the moulds into anaërophytic or aërophytic moulds.

"A. *Anaërophytic Moulds.* These are intermediate forms, a kind of transition between the last varieties of ferments and the moulds most developed. They appear only when the access of the air is hindered; for example, when they are plunged into the liquid or develope in the interior of a plant or animal: most of the varieties arranged in this category are true parasites.

"1. *Oidiate-form.* In this section we may place Achorion Schœnlinii (Oidium albicans, &c.). These forms develope in the living tissues filled with liquid, or indeed in the interior of pasty or mucilaginous substances. The mycelium then ordinarily remains little developed and produces at the extremities of the numerous joints what is known as a chain of spores. We meet with them in Favus.

"2. *Corruption-forms.* These occur in very thick substances.

"*a.* The form of Ustilago developes in thick starch, or in the interior of the culms of certain grasses very rich in sugar. It produces ramifications which terminate in a chain

of brown, spherical spores. This form belongs especially to Aspergillus.

"*b. Tilletia-form.* This appears under the same influences as the preceding form; but instead of terminating in spores with a continuous membrane, it terminates in spores with a reticulated membrane; for example, Penicillium.

"3. *Schizosporangiolen-form.* Here are found the Moulds whose filaments penetrate the atmosphere to terminate in conceptacles enclosing confined spores.

"B. *Aërophytic Moulds.* These are the true moulds. They grow on the surface of the substances which nourish them and in contact with the air. They produce a developed mycelium and ascending filaments, which bear the reproductive organs. In this section we find again three different forms.

"1. *Acrospores.* These plants bear free spores at the extremity of the ramifications. It is in this section that Penicillium and Aspergillus are found. They are produced when the nitrogenous soil is moist. One can, therefore, according to Hallier, raise, at pleasure, Penicillium and Mucor, or Aspergillus and Stemphyllium, as we sow, indifferently, the spores of one of these forms on a matrix dry or moist.

"2. *Thecaspores* or *Mucor-forms.* In this category is found the Mucor belonging to the genus Penicillium, and Stemphyllium, which is only a form of Aspergillus. The Thecaspores spring up only on very nitrogenous and dry soil. The organs of fructification of these species are in the form of a large vesicle containing many spores. Hence from *theca*, the name given to the envelope, *Thecaspores.*

"3. *Sexual-form.* The highest development of the mould family presents, in fine, sexual organs, some of which are called *Oogonia*, corresponding to pistils, and *Antheridia*,

corresponding to the stamens of phanerogamous plants. The contents of the Oogonia are fecundated by the Antheridia, and produce durable and winter spores. I will cite, as an example of this last form, Eurotium herbariorum, which developes upon the dried leaves of our herbaria, and which belongs to the species Aspergillus."

APPENDIX.

Cultures and Results.

Before entering upon the details of the following cultures, which were intended to be like those of Prof. Hallier, it should be stated, that his work* in which the apparatus used by him was described, was not to be procured in this country, and we were left to devise our own; which consisted of glass tubes from four to five inches in length and somewhat more than half an inch in diameter, closed at both ends by a perforated cork, for the admission of smaller glass tubes bent downwards at right angles and which were stuffed with clean cotton. Also open-mouthed phials of about one and a half fluid ounces capacity, whose apertures were closed by a cork doubly perforated, for the insertion of small glass tubes stuffed with clean cotton, for the extraction and renewal of the air, by suction, and for straining out any spores that might be floating about. Care was taken, if possible, to prevent the occurrence of anything that would have a tendency to vitiate the results of the cultures, although it must be confessed that had we known of the apparatus Prof. Hallier used, we should have endeavored to follow him more closely.

First Culture. On the 21st of April, 1871, sowed vaccine lymph on a paste made with a boiled solution of phosphate of ammonia and starch, in a clean, open-mouthed phial whose aperture was closed by a clean doubly perforated cork through which passed two glass tubes, stuffed with cotton moistened with alcohol.

On the 27th of April, examined a bit of the paste with B.5 (350 lin.). Saw a few small dark-colored points in

* "Gährungserscheinungen."

the liquid around the starch granules, none within them. It did not occur to test with an acid, so that it must be left doubtful whether this motion was molecular (Brownian) or not. Within the starch, as seen under C.10 (1200 lin.), were very many minute dark *still* points; many of the starch granules were, as usual, cracked and broken, while some were entire.

On May 5th, put a piece of kine-pock scab on the same paste, pressing it partially into the paste. These phials were allowed to remain unopened for more than a month, but no fungus whatever was to be seen. *Quere*—Were the fungi-spores from the outside and within (had any been admitted and included) destroyed by the vapor of the alcohol? Hallier says, "Alcohol is a disinfectant, destructive of spores."

SECOND CULTURE. May 5th, 1871, planted a piece of kine-pock scab on lemon. On the 9th found a fruitage, which was pronounced to be a bastard between Penicillium and Aspergillus (see Figs. 6, 8). In this culture the pellets of cotton had not been moistened with alcohol.

THIRD CULTURE. May 10th, 1871, placed pieces of kine-pock scab on boiled white of egg in two of the glass tubes previously described. In the first tube (1) the cotton was moistened with alcohol, in the second (2) it was not. On the 12th, examined with A.C.10 (425-1200 lin.), and found in tube 1 many moving points (probably molecular movement). In tube 2, the white of the egg exhibits signs of commencing decomposition. Saw specimens of Mucor mucedo (T. 3, Fig. 4), with burst sporangia and scattered spores. The Mucor is a morphism of Penicillium. May 16th, in the same tube (2) Mucor was found with interstitial sporangia (Fig. 7, T. 3). Eleven days had now passed, but no fungus was to be seen in tube 1, i. e. where the cotton had been moistened with alcohol. This state of things, in tube 1, remained the same for a week or more, while the white of egg in tube 2 was covered with fungi.

FOURTH CULTURE. May 24th, 1871, put a piece of kine-pock scab on starch moistened with a cold solution of phosphate of ammonia, into one end of the clean tube A, while at the other end of the same tube was placed a piece

of the same scab on paste made with a solution of phosphate of ammonia and then boiled. At the same time and place, a mixture of a cold and also a boiled solution of the above salt and starch, but without any kine-pock matter, was put into another similar clean tube, B. The small tubes, bent at right angles and plugged with clean cotton not moistened with alcohol, were passed through a perforated cork which closed each end of the tubes. I then sucked through the small tubes twenty times to strain out and prevent (if this would do it) the presence of any spores.

May 26th, 1871, saw a faintly red spot, which on the 27th became very distinctly of a pinkish color on the unboiled starch pellet in tube A. A similarly colored spot appeared on the bit of boiled paste at the other end of the tube (A). These colored spots were at a small distance from the pieces of scab.

On the 25th of May, I vaccinated three children and two infants from fragments of the same scab which I had used in the above culture. In three of the cases normal vaccine vesicles formed and matured on the 8th and 9th days; in one, the constitutional symptoms were very strongly marked. The vaccination failed in the infants, probably from too superficial puncture.

The Micrococcus of kine-pock, as well as those from Torula rufescens (the fungus which Hallier believes to be the source of kine-pock Micrococcus), are both reddish; the former, he says, is of a wine-red color, the latter of a darker red. The color before me on the unboiled pellet of paste in tube A, is of a pinkish color; that on the boiled pellet is a little more of a wine-red color.

To the above remarks upon the color, it may be objected—that fresh and active kine-pock lymph is colorless. So is newly crystallized biniodide of mercury; but if we touch one of these crystals with the point of a needle, the whole crystal becomes instantly of a beautiful red color, while it still remains biniodide of mercury, the arrangement of its particles only having changed; and in this manner the difference in color between vaccine-lymph and vaccine-scab, may be explained, as well as by the multiplicity of the Micrococcus.

May 29th, examined a bit of the reddish part on the unboiled paste, with C.10 (1200 lin.). Found Mycelium-threads and some very small spores, which proved to be Penicillium crust. with Macroconidia; the same fungus was found on the unboiled paste in tube A, not colored red. None of the characteristic cells of Palmella (which was suspected) could be found.

June 1st, again examined portions taken from the reddish colored spots, tube A, with C.10 (1200 lin.); find in that from the unboiled pellet large numbers of moving Micrococcus and starch granules, nothing else; and as the starch is white, the reddish color would seem to come from the color of the Micrococcus.

In a portion taken from a part not colored, sprouts of Aspergillus and Penicillium were found. In a part of the boiled paste from tube A, where the color was wine-red, find a great number of very active Micrococcus, and but very few starch granules. Examined, the same evening, parts of the contents of tube B (where there was no kine-pock matter); found very fine mycelium and many spores, apparently of Penicillium, but no bastard forms; some resting or still Micrococcus.

Fifth Culture. On June 2d, 1871, after carefully cleaning two open-mouthed phials as well as the tubes, sowed on boiled milk in A, kine-pock, by means of freshly charged quills, as also by vaccine scab; while in phial B, no kine-pock was placed, but only boiled milk.

June 5th, examined with C.10 (1200 lin.), what was on the cork of phial A, and found from the under surface of the cork very beautiful specimens of Aspergillus glaucus (Fig. 9, T. 3). Examined also the cork of phial B, and found only a few spores.

June 8th, examined some of the surface of the milk in phial A (where the kine-pock matter was present); find Arthrococcus lactis, also some whirling and rapidly moving bodies, probably vibrios. Examined also the under surface of the cork of phial B (where there was no vaccine virus); find only a few spores, supposed to be from Penicillium, but no Aspergillus: from near the surface of the milk were multitudes of fat-globules, some moving Micrococcus, but no Arthrococcus.

SIXTH CULTURE. June 14th, 1871. Repeated the fourth culture and with similar results. In two hours after the sowing I saw on the boiled paste, a reddish spot, which gradually faded away.

June 17, evening, examined with A.10 and C.10 (425. 1200 lin.) the contents of the tube, where bastard forms between Aspergillus and Penicillium, and what appears to be Oidium (Fig. 10, T. 3), were found.

SEVENTH CULTURE. June 17th, 1871, a cork was well washed in alcohol and a fresh surface given it. It was applied, with some Micrococcus of kine-pock on its under surface, to an open-mouthed phial, A, previously cleansed by washing in alcohol, and then with clean water, and set away. On the 18th, the same was done with the phial B, except that no vaccine matter was placed on the cork, and this phial placed alongside the other.

June 21st, evening, examined with C.10 (1200 lin.), the fungus on the under side of cork in phial A, and found much Penicillium (T. 3, Fig. 3) and some bastard forms between Penicillium and Aspergillus, but no perfect Aspergillus. On the corresponding part in phial B, no fungi were found.

June 23d, 1871. Having noticed, for a few days, that the unboiled paste in the tube where the kine-pock matter was placed on the 14th of June, was becoming of a reddish-brown color, it was examined with C.10 (1200 lin.), and we found that this color was owing to the conidia, which had fallen in great numbers from the sterigma. Oidium (Fig. 14, T. 3), and moving Micrococcus are also seen.

Professor Hallier says, "An Oidium first precedes Mucor, which could be but with difficulty distinguished from Torula rufescens." * * * "I have said that the Torula was an Oidium, that Oidium belongs to Mucor, which shows at the commencement, small, pale, reddish-brown conidia, closely like Torula rufescens."

June 24th, saw a pink spot on the white of egg (repetition of Culture 3), where on the 19th were placed pieces of kine-pock scab in tube A, and none in tube B. Strong mycelium was also seen shooting forth at a distance from the scab. Nothing in tube B.

June 25th, examined by reflected sun-light a portion of

the pale rose-red heap on the white of egg, which to the naked eye appears to be a Tubercularia. A bit of this commingled with water and placed on the glass-slide, resolves itself into a small cloudy spot, which under C.15 (1800 lin.), appears to be cells. See Fig. 11. Examined also the fungus, which proved to be Penicillium. In corresponding tube B, saw nothing.

June 27th, noticed in tubes A and B, some dark-colored spots on the boiled white of egg, where on the 30th was found under A, C.10 (425-1200 lin.), Oidium-like sprouts closely resembling Figs. 11, 12, of Professor Hallier's plate 1, and also Cryptococcus.

Eighth Culture. Rubbed up some kine-pock scab with pure and fresh pump-water and put a bit on the under side of a clean and freshly cut cork, which closed a phial in which was left a little of the water. On the 30th examined the above, which exhibited specimens of Penicillium.

On July 4th, 1871, I took vaccine-lymph from a young lad, some in capillary tubes, and some on a piece of clean glass or points.

July 5th, examined some of the above lymph, mingled with a minute drop of pump-water under C.10 (1200 lin.), and saw moving Micrococcus, on one of which it was thought could be seen what Hallier considers as the moving organ (the tail); this cilium appeared to be swinging in the direction of an obtuse angle. Some of the lymph blown out from the capillary tube was next examined, and although some small bodies were found, none were in motion.

Ninth Culture. July 5th, boiled a filtered solution of the phosphate of ammonia and added starch; boiled the starch with a small tube in the cork, so as to allow for the escape of steam and air; then, having put a portion of the vaccine lymph upon the drawn out end of the small tube, brought it just into contact with the paste; then slightly withdrew it. July 8th, a portion of the above was examined, but nothing found.

On the same day, under a fine light reflected from a house painted white (almost equal to the sunlight reflected from a cloud), examined a portion of the vaccine lymph from a capillary tube with C.10 (1200 lin.), and saw many bright bodies, some in motion, some quiet.

With C.15 (1800 lin.), saw Micrococcus, found in a liquid, where spores of Penicillium were put, about two months since: saw the cilia (or tails), a part of the time in motion, a part of the time at rest. This statement is made only to show the optical quality of the microscope used in the above examinations, and for comparison of the small bodies found in vaccine lymph and their instrument for motion.

July 10th, 10 A.M. Under bright light from the reflected sun-light, examined with A.10, the liquid paste (ninth Culture), and find Penicillium and bastards between it and Aspergillus. Above the surface of the liquid, the form is that of Penicillium (T. 3, Fig. 3), below the surface of the liquid, Oidium.

On the 13th of July, examined the under surface of the cork (Culture nine), and found Aspergillus.

July 14th, took some vaccine lymph in a capillary glass tube, from a young child vaccinated on the 5th instant, which was immediately examined (i. e., as soon as I could reach my office). With C.15 (1800 lin.), found many moving corpuscles, some of which strikingly resembled the two right hand forms figured in Dr. Beale's "Disease Germs, Their Real Nature," Fig. 39. Upon examining another drop of the same lymph, saw more cells, some resembling the spores of Aspergillus, others of about the same size, appearing like empty cysts, resembling those figured by Hallier, and presenting a trembling motion (T. 1, Fig. 5).

July 18th, 1871, examined with C.15 (1800 lin.), vaccine lymph taken (within half an hour) from the vesicle on the arm of an infant vaccinated ten days since; the vesicle was umbilicated, well-filled, and surrounded with the areola; many moving corpuscles, mostly round but smaller at one end, were seen. The motion resembles that of the swarmers of Algæ.

Tenth Culture. August 9th, 1871, planted vaccine matter on boiled white of egg, and placed in the tube; then sucked through the tube forty times.

Aug. 22d, the egg had become quite dry and covered with a fungus. Examined with A.10, on a dry glass slide, and found Torula chains, some free, some still standing on the branched hyphens (Fig. 15, T. 3).

In conclusion, while I would not presume to pass judgment on Professor Hallier's hypothesis upon the causes of zymotic diseases, I desire to state, that I have attempted a plain and simple statement of facts in the cultures made, and in the results obtained; but must confess that it was gratifying to find, under like circumstances, the same vegetable forms: viz. Penicillium, Aspergillus, the bastard forms between these two fungi, Oidium, Mucor and Torula, and to see the moving corpuscles in the fresh lymph of the vaccine vesicle: while it may be admitted, with Drs. Billings and Curtis, that "the laws of development are not sufficiently known to enable one to draw decisive inferences from such results."

EXPLANATION OF THE PLATES.

PLATE I.

Fig. 1. Zeiss system F. Ocular 2. Swarming Micrococcus of small-pox.

Fig. 2. Zeiss F.2. Cryptococcus forming in twenty-four hours from the Micrococcus of small-pox raised on glycerine, under an hermetically sealed bell-glass.

Fig. 3. Z. F.2. Mycothrix chains from the small-pox. In the links of the chain is seen a dark nucleus.

Fig. 4. Z. F.2. Swarming Micrococcus from sheep-pock, here and there Mycothrix chains, with a nucleus in each joint (*k*).

Fig. 5. Z. F.2. Quiet Micrococcus from the Munich kine-pock fluid.

Fig. 6. Z. F.2. Micrococcus from the sheep-pock, cultivated on sugared water. The Micrococcus cells are for the greater part grown to short Leptothrix or Mycothrix chains, Bacteria (auct).

Fig. 7. Z. F.2. The same culture. At the edge of the liquid and at the bottom of the vessel, the Micrococcus cells slowly swelling to large clear sporoids, ready to sprout, with one or more nuclei, after culture of fourteen days.

Fig. 8. Z. F.2. The same culture. The large round cells (Sporoids), which swim in the liquid, discharge their nuclei, which were formed from a plasm investing the wall.

Fig. 9. A sprout of Sporoid (Fig. 7), bearing in every branching thread a chain of at first oval, at last round spores, which become pale in the fluid and discharge their contents (Fig. 8).

Fig. 10. End of some sprouts at the brim of the vessel. At every termination is a chain of brown spores. There lay thrown out spores partly roundish, partly cladospore-like Septate (*c l*), partly very large and pale bodies (*m*), scattered in the fluid.

Fig. 11. Sprouts from the Micrococcus of Sporoids, germinating on the white of an egg. The spores were drawn out in an irregularly branched pencil.

Fig. 12. Micrococcus of sheep-pock, slowly swelling to cells able to sprout (Sporoids), which already, here and there, have put forth long shining Oidium-like sprouts.

Fig. 13. Spore-bearing thread of the sprouts from sporoids within the white of egg.

Fig. 14. Similar thrown off Tilletia-spores, getting pale in the liquid, forming Micrococcus.

Fig. 15. Sprouts from sporoids with Sporidesmium fruit at the brim of the vessel.

Fig. 16. Similar fruit with vigorous or swelling, divided spores.

Fig. 17. Another fruit branch from the same culture.

Fig. 18. Culture on paste and tartrate of ammonia. Micrococcus forming. The Micrococcus cells, in some places swollen to Sporoids (*sp*), multiplied largely within the soil. Single Mycothrix chains with nuclei were to be seen (*m*).

Fig. 19. Micrococcus in round, and changing to rod-shaped Arthrococcus, raised from the Micrococcus of the sheep-pock lymph on pear.

Fig. 20. Full-grown Arthrococcus from the same fruit, forming a close Mycoderm.

Fig. 21. Thread with six Sporidesmium fruits, from the white of egg culture.

Fig. 22. Culture from paste with the tartrate of ammonia. Fragments of a vegetative Mycelium thread.

Fig. 23. Pale red thread from the same culture, at the brim of the vessel beginning to fructify.

Fig. 24. Farther stage of fructification.

Fig. 25. Spore formation from the same culture.

Fig. 26. Sprouting Mycothrix-fungus, with tender Cladospore-fruit, from the same culture.

Fig. 27. Culture on lemon, Cladospore thread with Macrospore; (*m*), Monilia cinerea Bon.

Fig. 28. Rhizopus nigricans. Lemon culture, seen by an instrument of Beniche. System 4, Oc. 1.

Fig. 29. Penicillium grande from the same culture, produced from the sprouting thecaspore of Rhizopus. Shown by the same optical power as Fig. 28.

Fig. 30. Fruit-bearing branch of the same Penicillium, as seen with Zeiss F.2.

Fig. 31. Pleospore from Lolium perenne.

Fig. 32. Monilia cinerea Bon., obtained from Pleospore on pear. (*p*), a Puccinia-like conidia; (*m*), a Macroconidia.

Fig. 33. Oidium-lactis, appertaining to Monilia-Rhizopus-Botrytis.

Fig. 33 *a*. Monilia seen in a succulent pear, sprouted and forming a Penicillium thread; (*k*), Normal sprouts at the end.

Fig. 34. Sprout of Rhizopus nigricans Ehr., from an apple.

Fig. 35. Arthrococcus from the interior of a spore of Rhizopus borne on pear. Transition stage of Micrococcus to Arthrococcus.

Fig. 36. Spore-bearer (Hyphen) of Botrytis elegans Corda. From the Monilia raised on pear.

Fig. 37. Rhizopus nigricans Ehr. (as seen with a good lens), covered up by Penicillium.

Fig. 38. A single theca somewhat magnified, with Rhizopus twining up the bearer.

Fig. 39. Spore of Botrytis sprouting to a Rhizopus thread.

Fig. 40. Rhizopus nigricans Ehr., hanging downward from an apple peel, as seen by a good lens.

Fig. 41. Young sporangium of Rhizopus at the commencement of spore-forming.

Fig. 42. Sporangium of Rhizopus, the spores partly discharged through the cracked wall of the sporangium. From its bearer ascends a club-shaped mass of plasma.

Fig. 43. Wholly empty sporangium of Rhizopus. The wall is torn away and a shred (*w*) only is visible. The club-shaped mass of plasma is furnished with a new wall, with a distinctly two-fold outline (*c*).

Fig. 44. Amylum nuclei from the culture of kine-pock lymph on paste and phosphate of ammonia. The starch nucleus tears from the centre (*a*). In these rents, after a short time, the Micrococcus settle and largely multiply. After twenty hours, is seen the centre of the nucleus and often the layers filled with Micrococcus. Frequently the layers break into small fragments.

Fig. 45. Amylium nucleus from the same culture, some days after the sowing. The Micrococcus cells are mostly swollen to Sporoids, which, in part, are already sprouting, bursting and lacerating the nucleus until it is wholly dissolved.

Fig. 46. Example of Mucor mucedo Fr., as seen by a good lens, appearing on the cork in the same culture.

Fig. 47. Macroconidia of this Mucor raised from Aspergillus.

Fig. 48. Filled sporangium of the same Mucor, in the under part of the bearer Macroconidia, come to development.

Fig. 49. Discharged sporangium of the same Mucor ; the round stalk-cell (Columella) is seen.

Fig. 50. Culture of kine-pock on lemon, fourth day. Formation of sporoids from the Micrococcus, at *k* a sprouting sporoid.

Fig. 51. Macroconidia from the same culture.

Fig. 52. Culture of kine-pock fungus on the white of egg. Spores of Cladosporium (Oidium albicans auct.).

Fig. 53. Sprouting sporoids at *k*, from the same culture.

Fig. 54. Bastard between Aspergillus and Penicillium on the cork of the culture of kine-pock lymph in milk.

Fig. 55. Arthrococcus lactis, originating from the Micrococcus of the same culture.

PLATE II.

Fig. 1. Mucor from the culture of kine-pock on paste and phosphate of ammonia placed on a cork. *p l* are the swellings of the tender Oidium threads. At *v* is seen the Macroconidia of some chains united. *k* is the empty Mucor capsule with a round basal cell, and some attached spores.

Fig. 2. Cladosporium (Oidium albicans auct.) from the culture of vaccine-lymph on white of egg. *u*, Ustilago spore-chains not yet arrived at maturity, because they immediately shoot forth cladospores (*c l*), whose young spores (*o*) closely exhibit the nature of Aphthous-fungi.

Fig. 3. Aspergillus glaucus Lk., from the sowing of the vaccine-lymph on cork, at *s* the mostly hollow bearer of the pencil. *s p*, spores; *l s p*, these in air; *a s p*, the same in alcohol.

Fig. 4. Arthrococcus lactis, brought forth from the small-pox matter on boiled milk.

Fig. 5. A bastard between Aspergillus and Penicillium from the same culture.

Fig. 6. A fur of Cladosporium (Oidium albicans), appearing on lemon from the sporoids of Micrococcus of small-pox, under a good lens.

Fig. 7. The same fungus in fragments with Z. F.2. It showed Cladosporium chains (*c l*); Ustilago spores (*u*), in part very large and often septate (*u u*), large terminating round cells (*p*), partly with a mitre-formed end cell (*p m*); in part developed Septosporium-Stemphyllium fruit (*sp h*), partly becoming Pycniden (*p p*), with sporoids (*s p*).

Fig. 8. Sclerotium appearing on the cork of the sowing of small-pox on sugared water with phosphate of ammonia.

Fig. 9. Crypto-crystalline formation in Typhus-stool from Munich.

Fig. 10. Vegetable organisms appearing in the monkey's stool. (*a*), thread-sprout; (*b*), fungus cells; (*c*), Mucor spores; (*d*), Micrococcus-balls from the nearly dissolved spore-wall, still holding together.

Fig. 11. From the same, twenty-four hours after feeding with the spores of Rhizopus nigricans Ehr.; (*a*), Mycothrix chain; (*b c*), Micrococcus formation within the parent cell in two different stages; (*d*), conidia, one of which is about sprouting.

Fig 12. Yeast and thread formation in human excrement, after the inhalation of Rhizopus.

Fig. 13. Sporoid-formation from the mucous membrane of the throat.

Fig. 14. Micrococcus and therefrom produced Mycothrix-chains on epithelial cells.

Fig. 15. Fungus-formation in the monkey's intestine, on the fourth day after the feeding with Rhizopus.

Fig. 16. Fungus-formation in human fæces, sixteen hours after the feeding with Rhizopus.

Fig. 17. Yeast-formation in the monkey's stool, on the third day after feeding with Tilletia caries Tul.

Fig. 18. Fecundation-ball of Ustilago carbo, surrounded spirally with a thread.

Fig. 19. (*a*) Double-spore (Thecaspore) from an Ascus of Corticium amorphum: every half spore has pushed out a sprouting tube. (*b c*) Ascus of Corticium cultivated in glycerine on the object glass; all the spores have sprouted; the Ascus is broken through.

Fig. 20. Swarming Micrococcus of Penicillium crustaceum Fr., as shown by the immersion system 1-18 Mez, Oc. 4.

Fig. 21. Swarming Micrococcus cells of Corticium, drawn with 1-18, Oc. 4.

Fig. 22. Swarming Micrococcus (from Munich typhus stool), belonging to Rhizopus nigr. Ehr., drawn under the same system.

Fig. 23. Swarming Micrococcus of Mucor mucedo.

Fig. 24. Brown hair with Sclerotium Beigelianum. Z. system D. Oc. 2.

Fig. 25. A small fragment of such, under Z. system F. Oc. 2.

Fig. 26. Sprouting cells of a Sclerotium bursting in moist air; at (*k*) a longer sprout; at (*b*) a fructifying pencil of Penicillium, Syst. F, Oc. 2.

Fig. 27. Artificially raised Sclerotium from Aspergillus, near by a fruit pencil (*a sp*) from light hair. Z. syst. D. 2.

Fig. 28. Some cells of such Sclerotium, partly sprouting. Z. F. Oc. 2.

Fig. 29. Oidium of Rhizopus; the cells have partly discharged their nuclei, which, outside the parent cell, form nuclei and Mycothrix chains.

Fig. 30. Sproutings on the joints of Favus-fungus like cryptococcus.

Fig. 31. Development of spores of Aspergillus from the end twigs of Sterigma, at *x*.

Fig. 32. The same from Penicillium.

Fig. 33. Origin of Conidia from side-sprouts of the thread-joints of a Pleospore.

Fig. 34. Development of Micrococcus from the Conidia, and joints of the same fungus.

Taf. I.

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Wagenschieber sc.

TABLE III.

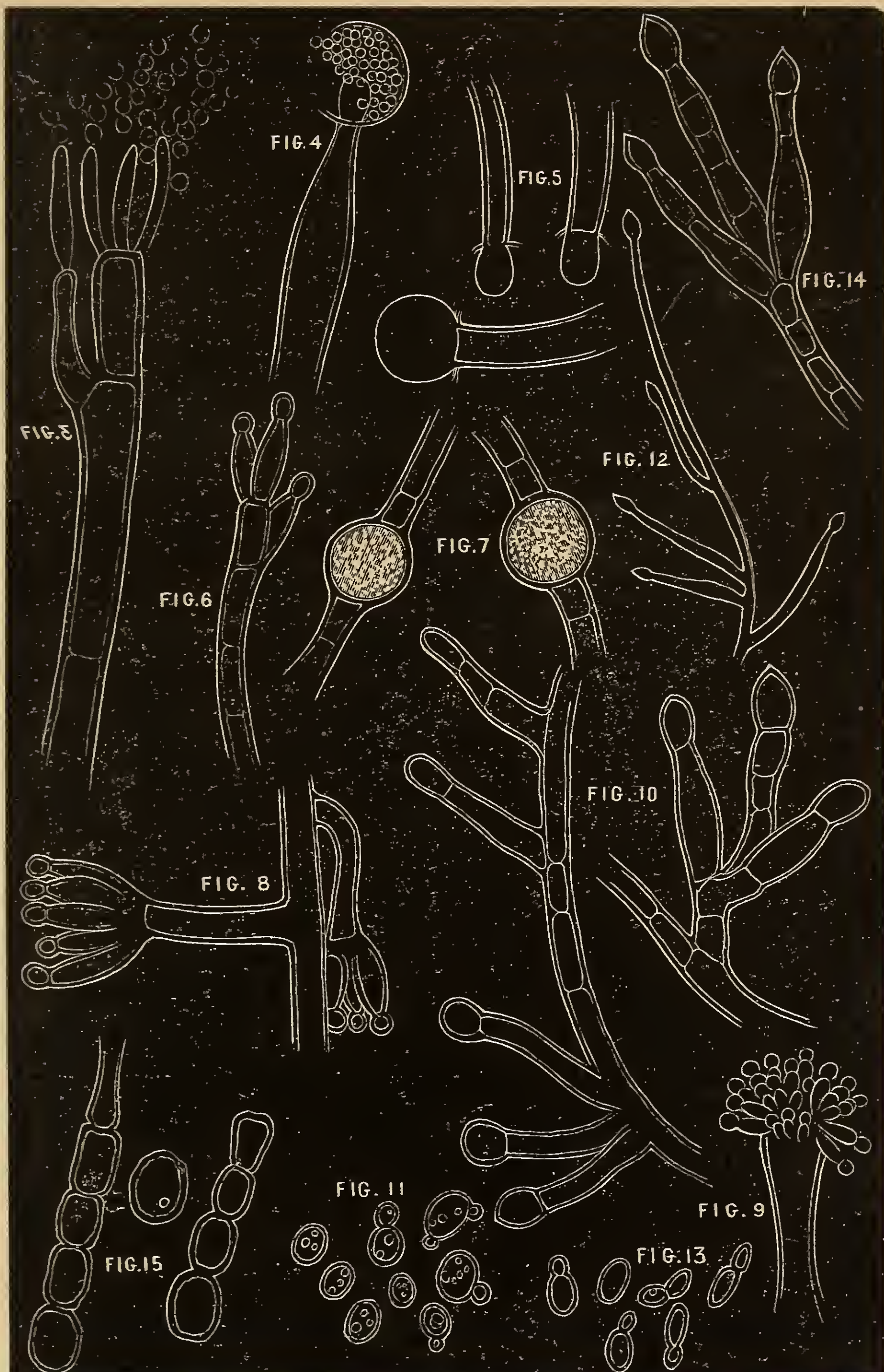

FIG. 3
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FIG. 10
FIG. 11
FIG. 12
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FIG. 14
FIG. 15